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FILM, SURFACE, AND BULKY COLORS AND THEIR INTERMEDIATES[1]

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I. INTRODUCTION

Katz has made us familiar with a mass of detail concerning the various modes of appearance of colors, which were first hinted at by Hering.[2] There is a general tendency among psychologists to accept Katz' detail, but there seems to have been no definite effort made to confirm it from the foundation. This work of verification, desirable in itself, is all the more important because Katz' interest lay not so much in phenomenology for its own sake as in other and more special problems. For these reasons we have attempted a further study of those modes of appearance which seem fundamental.

[1] From the Psychological Laboratory of Cornell University.

[2] D. Katz, *Die Erscheinungsweisen der Farben*, 1911.

In the opening chapter of his book Katz describes and compares eight modes of appearance: (1) film colors (*Flächenfarben*),[3] (2) surface colors (*Oberflächenfarben*), (3) transparent plane colors (*durchsichtige Flächen*), (4) bulky colors (*Raumfarben*), (5) mirrored colors (*gespiegelte Farben*), (6) lustre (*Glanz*), (7) luminosity (*Leuchten*), and (8) glow (*Glühen*). Film colors, surface colors, and bulky colors are the most important, because all other modes of appearance may be regarded as forms or combinations of these.[4]

In comparing surfaces and films, Katz takes paper as a typical example of surface, and the colors seen in a spectroscope as a typical example of film, although many other examples are also given. The observable differences between film colors and surface colors he groups under five headings: localization, texture, orientation, configuration, and aesthetic effect.[5]

1. *Localization.* "The spectral color of the usual apparatus is not localized with the same definiteness at a precisely determinable distance from the observer as the color of the paper." In saying that the film color is indefinitely localized, Katz does not mean that its distance varies from moment to moment. He says emphatically that, whether the observation is continuous or recurrent, with constant *Einstellung* of the eyes the distance of the color in no way changes. By indefinite localization he means positively indeterminate. It is true that probable limits may be set; but these limits vary with the *perceptive* conditions under which the film color is seen. For example, the limits lie much farther apart for the film of the sky than for that of the color seen in the spectroscope. Within these extraneous limits, the localization is positively indeterminate.

2. *Texture.* There is a baffling contradiction about the appearance of the film color. It seems to suggest depth, but proves in fact to be impenetrable. "The paper has a surface in which its color lies. The plane in which the spectral color stretches through space before the observer does not possess a surface in the same sense."[6] One almost feels able to penetrate more or less deeply into the spectral color, whereas with the color of paper the gaze is forced to stop at the surface. The openness, the softness, the insubstantiality which the spectral color betrays is, however, not of such a kind that one can speak of a clearly bulky mode of appearance of the spectral color, that is, of a visible filling of space in three dimensions, or of any sort of colored transparency. Like the color of paper, the spectral color is bidimensional and screens the space behind from view.

3. *Orientation.* There is a marked difference in the orientation of the two types of color. The surface in which the color of an object, such as paper, seems to lie can take any orientation to the line of regard. The film color tends always to assume a frontal-parallel position. Indirect vision of course constitutes an exception to this rule, and there are certain other exceptions. For example, the apparent orientation of film colors is greatly influenced by the orientation of neighboring objects of *perception*. Katz describes an experimental arrangement whereby a bit of the sky, viewed through a cardboard tipped at an angle, takes on an apparent

[3] We could find no entirely satisfactory term for the appearance of the "completely reduced" color. Katz' word, *Fläche*, does not suggest an English equivalent. After trial of several words and after consultation with our *Os* we decided upon the term "film." Our "film," however, as is shown in the text, is not absolutely identical with Katz' *Fläche*.

[4] *Ibid.*, 6.

[5] *Ibid.*, 7.

[6] *Ibid.*, 7.

orientation about midway between that of the cardboard and the usual frontal-parallel position.[7] In spite of such susceptibility to secondary influences, the film color possesses in and of itself a tendency to appear in frontal-parallel orientation.

4. *Configuration.* The surface of a body may be either smooth or ridged and curved in various ways. The film color on the contrary always lies in a practically smooth flat plane.[8]

5. *Aesthetic Effect.* The spectral color has something delicate about it, something more pleasing aesthetically, than the color of paper.—

This completes Katz' list of differences between film colors and surface colors. In connection with the third difference, that of orientation, Katz discusses, however, another difference, which he seems to regard as more fundamental than any of these five. Surface colors not only conform to the surfaces of objects, but they seem also actually to be stable properties of the objects. We can shadow or illuminate one and the same surface. A shadowed or illuminated film, however, is simply a new film color. Surface colors are color-qualities of objects. Film colors are unreferred colors, colors as such.

"So far as the psychological conditions are concerned, the consciousness of having an object before one on which the colors arise is for the perception of surface colors, I venture to say, of decisive significance..... As compared with the great influence which the inner *Einstellung* exerts on the occurrence of this or that mode of appearance of colors, one may designate as almost irrelevant the physical source of the radiation setting up the impression."[9]

The relations between film colors and surface colors can easily be studied in the laboratory, because any sort of surface color can be reduced to film color by the use of a screen with a small hole, or (still better) of a double-screen.[10] The exact form of double screen is not important provided the following essential conditions are satisfied. (1) The screen must cover the object completely except for the part seen through the opening. (2) At the same time it must not permit any structure or grain that may possibly be present to be recognized. (3) It must not permit any non-frontal orientation of the portion of surface seen to be recognized. (4) The hole must not be too large.

Between surface colors and film colors, all possible phenomenological transitions occur.[11] Many *Os* experience such a transition when monocular observation is substituted for the usual binocular vision. An intermediate which approaches more nearly to the true film can be secured by placing before the eye a lens too strong to be overcome by accommodation.—

The third of the essential modes of appearance of colors is that of the bulky colors. These differ from film and surface alike in that they are tridimensional and partially transparent. They seem to fill a *definite space* in its three dimensions. "According to my observation," says Katz, "they show this property in a distinct way only when they are at the same time in a true sense transparent," that is, so long as objects are actually seen through them.[12] When no object can be distinguished through the color, Katz would call it a film color.

Bulky colors, like surface colors, can be reduced to film colors. For this reason, Katz considers the film as the original mode of appearance of colors, and regards all other modes as derived from the film.

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[7] *Ibid.*, 75.
[8] *Ibid.*, 12.
[9] *Ibid.*, 9.
[10] *Ibid.*, 9 f.
[11] *Ibid.*, 9.
[12] *Ibid.*, 17.

The upshot of Katz' whole discussion is, evidently, that the film represents the ultimate stuff of vision. Color or light in the psychological sense is filmy. If we assume with Katz that the film is preperceptive, it follows that localization, orientation, and configuration cannot be attributed to it as such, and appear under experimental conditions only because and in so far as secondary perceptive conditions are introduced by the experimental arrangement. Going beyond Katz' empirical statements, we might lay down the following laws. (1) The film is not localizable. If it is localized, that is merely because, *e. g.*, we are familiar with the instrument through which we are observing. (2) The film has no orientation. If it appears always in a frontal-parallel relation to us that is simply because, in making it an object of observation, we give it the easiest and most obvious orientation; hang it, so to speak, on or across the line of regard. (3) The film has no configuration. As sheer sensory quality, it must show as a plane of homogeneous color.

Before the psychological status of the film is fully established, these statements must be confirmed experimentally by other *O*s. Psychologists have perhaps tended to take Katz' statements to be systematic; they are, in fact, merely empirical. The reader of the introductory chapter can scarcely help noticing many apparent contradictions. If, *e. g.*, Katz is correct in assuming that the fundamental difference between surface and film colors is that surface colors are ascribed to objects and film colors are not, in what sense can he assert the existence of transitions between film and surface? Surely the source ascribed to the color either is or is not an object.

Our experimental series were directed toward answering the following questions: (1) Are there intermediates between film and surface? What is their phenomenology, and in what sense may they be regarded as intermediates? (2) Can it then be shown experimentally that the object-consciousness is sufficient of itself to ensure the perception of surface color? (3) If the film color really is the ultimate stuff of vision, what happens to it that changes it to surface color in the one case and to bulk in the other?

The *O*s were Dr. L. B. Hoisington (H), assistant professor of psychology; Dr. H. G. Bishop (Bi), instructor in psychology; Miss C. C. Braddock (Br) and Miss G. Adams (A), graduate students in psychology; and the writer (M). All had had training as *O*s in previous psychological investigations.

The apparatus used for reducing surface to film was a double screen, similar in essential details to that described by Katz. It consisted of two entirely separate screens, constructed as follows. To a wooden frame, 71 cm. x 56.5 cm., was tacked a sheet of heavy grey cardboard which approximately matched in brightness no. 10 of the Hering series of grey papers. In the cardboard was a circular hole, 25 cm. in diam., whose center was approximately 31.5 cm. from the bottom of the screen, 25 cm. from the top, and 35.5 cm. from each side. To the back of each screen was fastened a strip of wood so cut that it formed a slot into which could be dropped smaller sheets of the grey cardboard. In the center of these smaller screens was a hole. The holes varied from 2 cm. to 20 cm. by steps of 2 cm., and were numbered from 0 to 9. There was a pair of every size, one to fit each of the two members of the double screen. The back of each hole was ground down with emery paper to present a sharp, knife-like edge. When one of the small screens was fitted into the slot, the small hole was concentric with the large hole.

The experiments were performed in a dark room, approximately 6 x 2.5 m. It was lighted from above by three 'daylight' lamps, arranged in a single row along the middle of the ceiling. The table to which the apparatus was fastened was set up at the side of the room, almost exactly midway between two of the overhead lamps, so that the screens and the stimulus behind them received an almost equal amount of light from the two sides.

There was practically no brightness-contrast between the fore and back screens in these experiments. The screens, being entirely separate, could be set at any desired distance from each other, but in all regular series they were 25 cm. apart. Other distances which might be varied were the distance of the stimulus from the back screen and the distance of the *O* from the fore screen.

From a few preliminary trials it seemed probable that reduction is favored by increased distance of *O* from the fore screen and by increased distance of the stimulus from the back screen, just as it is favored by a relatively wide distance between the two screens themselves. In all regular series, the *O* sat at a distance of 1 m. from the fore screen. This position was kept constant by the use of a fixed headrest.

II. Surface to Film

Series I. Effect of Varying Size of Hole

Series I was originally planned to answer our first question: Are there intermediates between film and surface?

The colored stimuli were cloths stretched very smooth on wooden frames, and Milton Bradley papers pasted on sheets of stiff cardboard. These could be fastened by thumb-tacks to a wooden and cardboard frame, so that a quick and easy shift from one color to another was possible.

When viewed without the double screen, these colored stimuli were, of course, ordinary surfaces. When they were viewed through the very small holes, their appearance approximated in most respects to the film as described by Katz. There were certain differences, to be discussed in detail later, but these need not concern us here. It seemed plausible to suppose that by varying the size of the holes, we should find, somewhere between the largest holes, which gave surface, and the smallest, which gave an approximation to film, some of those intermediates to which Katz refers.

The surfaces, like the films to which they were to be reduced, were flat and possessed a frontal-parallel orientation. The criteria of the true film were assumed to be indefinite (*i.e.*, only secondarily motived) localization; bidimensionality; loose, soft, insubstantial texture; and complete lack of objective reference. We purposely omitted aesthetic effect, because we could not agree with Katz in regarding this as an observable difference on the same level as the others. All *O*s, however, occasionally exclaimed at the beauty of the film colors, in spite of the fact that aesthetic effect was not mentioned in the instructions.

It will be remembered that Katz' *O*s fixated the edge of the hole in the back screen,[13] because this fixation was most natural and convenient and was moreover adequate to the purposes of his experiments. Since, however, we were to ask for reports of localization and dimensionality, it seemed best to instruct the *O*s to fixate the color under observation.

[13] *Ibid.*, 38, 73, etc.

The original instructions for H and M read as follows. "After the usual 'Ready, Now' signal, a grey screen and a brown disc will be exposed for 3 sec. Fixate the center of the disc. Immediately after the exposure, characterize the disc as either (1) surface, or (2) film, or (3) intermediate. Then describe it in terms of texture, apparent localization, dimensionality, and objectivity, and any other striking characteristics. In subsequent exposures, it is unnecessary to repeat in detail items that remain the same as in the previous exposure. Report fully whatever changes." The instructions for the other *Os* were the same, except that they were not asked to report on more than *two* descriptive characters at a time. In addition to the typewritten instructions, the *Os* were given typewritten slips with the following definitions. " 'Intermediate' means like surface in some respects and like film in others. 'Objectivity' means attachment or reference to a particular object, such as a board, a piece of cloth, a piece of colored glass, etc. 'Localization' means position, either (1) before the screen, or (2) at the screen, or (3) behind the screen, with a rough estimate of distance in the first and third cases. 'Texture' means material nature or substantial character, as transparent, translucent, opaque, hard, soft, solid, etc. 'Dimensionality' means extension solely in two or definitely in three dimensions." The term "mixed" was soon substituted for "intermediate," because under the conditions of experimentation no true (psychological) intermediates between film and surface were found. Later the term "mixed" was also dropped. For greater clearness and freedom from suggestion the definitions of terms were also revised, to read as follows. " 'Objectivity' means the conscious reference of the observed color at the time of observation to some concrete thing. 'Localization' means a given position, whether constant or temporary, of the observed color in the third dimension of space. 'Texture' means material nature or substantial character of the observed color. 'Dimensionality' means extension of the observed color solely in two or definitely in three dimensions."

We at first attempted to tabulate the results as fast as they were secured, but soon found that their complexity made a tabular mode of arrangement practically impossible. We shall instead treat of the observed phenomena in groups. It will be noticed that there is considerable overlapping between the groups.

Group I. Surface Colors

The phenomenology of surface colors presented no real difficulties. The descriptions were simple, brief, and consistent.

Localization. Bi (9)[14] "About a foot and a half behind the screen." A (7) "It looked about two or three feet from the screen." Br (8) "Localized at a slight distance behind the back screen." H (9) "Surface behind the screen at some distance, a meter or so." H (9) "About .5 m. behind back screen." M (7) "Surface, quite definitely localized behind the back screen, though I can't give the exact distance in feet and inches."

Objectivity. Bi (8) "I could make out the inequality of the cloth, the weave, the fabric." A (9) "Rough green cloth." Br (8) "More uniform than cloth, more like paper." H (7) "Fairly definite object, cloth, or paper, or almost anything with a grain to it." M (9) "Opaque soft cloth."

Texture. Bi (9) "Weave of cloth." A (8) "Rough. I can see ridges, very close together, blurred." Br (7) "Smooth shiny cloth with darkish

[14]Numbers in parentheses refer to the number of the hole used.

grain." H (9) "Coarse woven cloth, slightly fleeced, so that texture is just a little obscured. Solid. Opaque." M (7) "Solid, opaque, soft cloth with ridged weave."

Dimensionality. Surfaces were always reported as bidimensional.

Group II. Film Colors

The *O*s were unanimous in describing the film color as indefinitely localized; but as regards the stability of the localization of film colors we obtained results that at first seemed opposed to those of Katz. It will be remembered that he combats the contention of Hillebrand that indefinite localization involves shift of position,[15] and asserts that the film color does not change its distance, although it is positively indefinite in localization. Hence we were surprised to get occasional results like the following.

H (3) "One can do stunts to those things, so far as localization is concerned. They are instable. One can read all kinds of meanings into them." H (2) "Localization is shifty. At first it seemed to fill all the space behind the screens; but I took a localization attitude later, and it was easy to throw the thing farther back." H (1) "Slightly shifting localization. I don't mean that at any time I could say just where it was, only that sometimes it seemed nearer and sometimes farther." H (4) "Localization tends to shift from at the screen to far behind." Br (4) "It lay behind the back screen, came forward a bit, and then seemed to shift up to the fore screen." Bi (0) "I'm not sure whether it was always in the same place or not. I think there was a little play back and front. Not much."

The opposition between these results and those of Katz was, however, apparent rather than real. Katz says that the localization of the film color is stable with *constant Einstellung of the eyes.* With a little more practice our *O*s soon discovered that the apparent shifts in localization were due to shifts in fixation. It will be remembered that the original instructions read "Fixate the center of the disc." These instructions in a sense demanded the impossible, because there was nothing there to fixate.

H (1) "When you try to fixate, you have just that meaning of looking for something you can't see. You tend to stare. The eyes are held at an immense strain." Bi (6) "Almost impossible to fixate the center of the disc. My accommodation is for two or three different places!"

In hunting as it were for a place to anchor, the fixation naturally shifted about, and the film color, having no localization of its own, followed the changes of convergence. It therefore seemed advisable to alter the instructions with respect to fixation to read: "Regard the white spot at the lefthand edge of the screen opening as a fixation point and begin every observation with your eyes directed to that. You need not, however, feel obliged to maintain this fixation throughout the period of exposure." Under these instructions the *O*s made more accurate reports of their fixation, and it was found that apparent shifts in the localization of the color were always correlated with shifts of fixation.

[15] *Ibid.*, 11.

Bi (7) "I can't tell where it is, unless I hunt for it by changing fixation. In that case, the film seems to be anywhere I look for it, so long as I'm looking behind the second screen." H (1) "If I look at the back screen, then the color is beyond it. There is no color between me and the fixated screen. If, however, I try to fixate the color, then it is absolutely not localized." Br (1) "It jerked forward two or three times and then back again. I think these were eye-movements." A (3) "Every time my fixation shifts, the color shifts farther away or closer. I don't know how far away it was. All I could tell was that it was shifting." H (3) "It doesn't change a bit so long as one holds fixation." M (1) "The whole thing is perfectly stable so long as fixation is held, but tends to shift with fixation." M (1) "Color might be either at the back screen or slightly in front of it, or almost any distance behind; not that it ever seemed to be precisely in any of these positions. It remained absolutely stable so long as fixation was held, but shifted with shifting fixation, in this sense, that when I looked for it in one place it always seemed to be somewhere else. I never saw it actually shift. I never saw it in motion."

In regard to the tendency of the unlocalised film color to link itself with perceptive experiences in the visual field, our results also agree with those of Katz.

H (1) "It's just out there, apparently localized only in connection with some other experience." H (1) "Meaning of forcing the gaze at nothing in particular. In that sense you may say that the thing is not localized at all. But even so, one must force the gaze beyond the second screen and that seems to carry the meaning that the color is beyond that. One can't focus the eyes at a point and say the color is there. In that sense the localization is absolutely indefinite." Bi (4) "When I fixate the edge of the disc, I can see that the brown is behind that edge somewhere. No front or back limit to the brown."

In certain preliminary experiments, in which we tried to determine what length of exposure was most convenient for observation, we found that this tendency of the film color to link itself to the nearest perceptive object, the screen, is enhanced by very brief exposure times.

H "With the shorter exposures, the tendency for the color to be linked up with the back screen is a little more pronounced. With the 1 sec. exposure, it became almost a part of the screen, only it had no definite surface, nor any objective reference; but it belonged to the same depth level."

Thus our results with regard to the localization of the film colors are in full agreement with those of Katz. This was not the case with dimensionality. The colors observed were not unequivocally bidimensional.

H (1) "Comes right up to the screen, but extends back indefinitely." H (2) "Depth, but not penetrable." Br (1 B)[18] "I couldn't look through it. It wasn't gauzy, but it seemed to have a bulkiness about it. I shouldn't say definitely tridimensional. Indefinitely so." H (1) "Tridimensional?

[18]Letters following the numbers in parentheses refer to the colored stimuli. When no letter appears, a dark brown rep was used as a stimulus. Other backgrounds used in experiments from which these introspections are quoted were bright green cheesecloth (G), red-orange cheesecloth (O), purple lawn (P), and light brown gingham (B).

Hard to say. It might have been an unlocalized bidimensional something. The fact of its not having a beginning is the reason for calling it tridimensional. It may be due precisely to the fact that it was not localized." H (1) "I fixated the white spot practically all through the exposure. Under those conditions, I should call the color bidimensional. At least I didn't see any tridimensionality." M (1) "Not definitely tridimensional nor definitely bidimensional. I should have been inclined to call it tridimensional, but could not do so, because it has neither front, back, nor middle. My reason for wanting to call it tridimensional is that it is not localized in a single plane; but calling it tridimensional would not really help because it is not localized in any bulk of space either." M (1) "Tridimensional in the sense that very dense fog and thick, non-transparent dust clouds are tridimensional. Certainly not tridimensional in the sense that fluids in bottles or masses of jelly or blocks of colored glass are tridimensional. The brown color here had no beginning and no ending in the third dimension." A (1P) "I think I'd call it three dimensions, but I'd have to stretch what I mean by that. It certainly is more than two dimensions." A (1P) "I wouldn't say it was definitely in three dimensions. It was very indefinite. It's different from what I'm in the habit of calling dimensions. Dimensions are usually either solid or they've got some boundary that's flat or that blocks the thing off in some way. This sticks out in front, but it sticks out in such a vague way. It's not solid, and it hasn't any boundary." Bi (oB) "Not sure about the dimensionality, beyond the fact that it was spread out." A (oG) "I can't localize it, and I can't tell about its dimensions either." H (1) "I don't know. It certainly both is and isn't tridimensional, if one can make any sense out of that. Bidimensional, I believe." H (1) "One is left with the meaning that you don't know whether the thing is tridimensional or not." H (1) "How in the world can you say anything about the dimensionality of a thing if you don't know where it is?" H (1) "Bidimensional. I believe that the difficulty with the dimensionality is one of localization primarily." M (1B) "Bidimensional in the sense of having no definite extent in the third dimension, but tridimensional in the sense of not being localized precisely in one plane."

From these and similar observations it would appear that the phenomena with which we were dealing were neither bidimensional nor tridimensional. They were merely extended, without dimensions in the geometrical sense. We might almost have anticipated this result. For, after all, dimensionality in that sense is already a *perceptive* character. If a color is localized in one plane we call it bidimensional; if it is localized in more than one plane, we call it tridimensional; but if it is not localized at all, on what basis can we call it either? In describing the color as bidimensional, the *O*s had reference to its smoothness and impenetrability; in describing it as tridimensional, they had reference to its softness, looseness, and "invitation to penetrate." In either case, what the *O*s were describing was not dimensionality proper, but the *predimensional* nature of visual quality. So in the light of our results it seems evident that the product of the "reduction" of surfaces cannot be called bidimensional in precisely the same sense that a surface color is bidimensional. The film is bidimensional in the negative sense, in that it is not yet positively tridimensional. It is also tridimensional in the negative sense, in that it is not yet positively bidimensional.

The question of objective reference gave no difficulty. All the *O*s reported that the pure film colors had absolutely no objective reference.

The film colors had very characteristic textures, but the *O*s found considerable difficulty in describing these except by the use of perceptive analogies.

A (1) "It's hazy, but not haze. In the woods where there's been a fire and smoke lies in a thin layer on the ground you'd have something like this." H (1) "Like something viscous or gelatinous, if you could see such a thing without a definite front face." H (3) "Haze, solid, translucent, tridimensional,—like colored glass,—and yet, no! No analogy under the sun is exactly correct. The light is what bothers me. I don't see light through it nor light reflected from surface, nor is the thing especially luminous." Bi (oP) "It had a curious soft penetrableness." A (oG) "You wouldn't say it's luminous, but there's light in it in some way. It wasn't reflected from it." H (general observation) "The darker ones are always more like looking into a colored hole, whereas the brighter ones seem glowing." M (1) "Opaque in the sense of shutting off space behind, but not opaque in the way that a solid is opaque, for the shutting off did not occur at any particular place. I could look into the color, but not through it." M (1) "It was not opaque as solids are opaque. Yet it could not in any sense be seen through. The meaning of penetrability seems to be associated with the effort to gauge localization." Bi (5 O) "Self-radiant, somehow. I'm sure that if I didn't force it down by careful fixation it would be a tremendously glowing color. It's something like the glare we get at night when we approach automobile lights. It has the same beaming, dense character." Bi (3) "Haze points too much toward fogginess. This is beautifully clear. This is too dense for thin smoke.....It's like looking into a window from a lighted room into the dark. The dark comes clear up to the window, but you don't know where it leaves off. Same kind of thing here exactly." Bi (2) "No texture in the usual sense. Foglike, but not so dense as fog. Localization is uncertain. It seemed to stand behind the hole in space. There is a penetrability about a fog that this does not have." H (1 O) "I don't know whether it's translucent or not. It's something like translucent. Rather like dense gas, though not exactly like any I've ever seen." H (oP) "It's just a patch of color that isn't objective, and yet one tries to apply textural terms to it. If you have a texture, it must be a texture of something."

While in general there is no harm in saying that the tissue of the film color is soft and insubstantial, our reports indicate very clearly that the film is *pretextural* in the same sense in which it is predimensional. Texture is a perceptive character, and therefore cannot in its ordinary sense be attributed to a sensory quality. Texture, like dimensionality, seems thus to be bound up in some way with fixation.

H (3) "It's easy for that to be two different things: (1) a transparent (I don't like the word glassy) thing that becomes gradually less translucent, and then gets so dense that you can't see any farther into it. The thing shifts from that to (2) something at or almost on the screen, lustrous, almost luminous. I have never seen anything like it. I'm certain these two things depend on a shift of fixation."

Intermediates

No true psychological intermediates were found, under the experimental conditions of this series. With the holes inter-

mediate in size between those giving ordinary surface and those giving pure film, we obtained appearances that involved both simultaneously. These were usually described as a surface, seen more or less obscurely, and more or less definitely localized behind a semi-transparent medium of the same color. Similar appearances were frequently obtained with holes that at other times under precisely the same external conditions gave ordinary surface on the one hand, or pure film on the other.

The phenomenological intermediates found with different colored stimuli and various sizes of holes differed in the localization and texture of the surfaces and in the depth and density of the obscuring media. Strictly speaking, they did not fall into sharply defined classes. For convenience, however, we have classified the results more or less arbitrarily into four fairly distinct groups. Starting with surface as given, we have the following.

Group A. Soft Surface

Br (5) "More like surface than anything else, a surface that is a little thick; like a woolly surface, loose-woven so that you can look into it; like thick, blankety cloth." M (9B) "Cloth. Bidimensional. Definitely localized behind screen at uncertain distance. Very soft, velvety surface." H (7) "Solid without being highly resistant, like cut liver. Velvety comes nearest, but—! Something that by vision shows it is soft to touch. Bidimensional with a hint of tridimensionality." A (7) "Like a woolen sweater or a chinchilla coat or soft snow." Bi (9) "Nearly surface. The color is one foot or more behind the screen. Surface is irregular mottling of color."

Group B. Veiled Surface

Br (8) "Surface with a slight film over it." Bi (7) "Surface, 1 ft. behind screen. Out in front there was a little bit of brown haze." Br (8) "I don't know where the front or back of the film lies, but I do know where the surface component is." A (6) "As if you had a surface color and a haze in front of it." M (7) "Plainly cloth, not quite definitely localized. Suggestion of very thin, indefinitely localized film of brown haze somewhere in front, perhaps contiguous with the surface, perhaps detached from it. The cloth itself appears velvety, *i. e.*, the color is not localized in one plane, and still less is there any distinction of localization of separate threads to form a clean-cut cloth texture." H (7) "Very thin cloud or gauze before a solid, opaque, slightly irregular surface."

Group C. Fogged Surface

M (6) "Smooth, stony surface. It might be cardboard, stone, wallpaper, or cloth. The brightness pattern suggests cloth, but the surface appears too smooth; or rather, it lacks the definite distinction of localization of separate threads, which is characteristic of cloth. The surface as a whole is not quite certainly localized, and appears to lie behind a medium which is brown but nearly transparent, and is of indefinite depth and uncertain localization." Br (6) "Sort of brownish haze, looking back onto surface. Surface hard to distinguish." Bi (6) "I felt as if I could make out a little of the fleckiness of the cloth. Hazy foreground of color, as if at the back limit of it there was a surface; rather definitely localized. Localization of the haze is uncertain. All this is somewhere behind the back screen." H (5) "A surface, adhering to an object, and then something

else in front, just as one might see through a vaporous film. The tridimensional seemed set off from the bidimensional objective. The surface was localized at about 25 cm. behind the screen." A (6) "It looks like a surface behind, and thick haze in front. The surface looks like soft cloth."

Group D. Immersed Surface

M (4) "A surface, or at least something opaque, behind a medium of decreasing transparency. The medium began at or close behind the screen, and extended to the unidentified object, which was rather doubtfully localized, somewhere between 10 and 20 cm. behind the back screen." M (2) "Very indefinitely objective. Close behind the screen, not beginning in any definite plane, was a vaporous brown color, very dense and very still, almost so dense as to suggest a viscous liquid. This vapor was nearly transparent where it began, but became progressively less so farther back. Behind it at an indefinite distance, not very far, however, was a gelatinous or perhaps solid something whose texture was completely obscured by the medium in front." A (6) "Something with a haze in front of it. Too rough to call it surface, *i. e.*, not smooth enough across." Br (2) "More haze than surface. It stops somewhere indefinite." Bi (5) "Haze seemed to end in a surface, but I didn't see the surface, either." H (4) "Solid and opaque behind, but with a gradual transition through cloudy to a thin, feathery gauze, and to a vaporous almost-transparency. Tridimensional with a hint of surface at the back. The haze obscures too much to allow objective reference."

It is clear that what we have phenomenologically before us, in this series of observations, is the gradual break-down of a visual object. We begin with the colored surface of a determinate thing, and we end with a sheer quality of color that is not the color of anything. Phenomenologically, we agree with Katz, the transition is continuous. The object of which the color is the surface becomes more and more vague, indefinite, unrecognizable, until only a vestige of objective surface, a hint of the surface of an indeterminate something, is left; and finally this last vestige of objectivity itself disappears.[17]

Psychologically, however, the picture is very different. At first we have the fairly simple perception of the surface of a material object;[18] this perception grows more complex, as the surface recedes and the haze before it becomes prominent; in the stage which we have distinguished as Group D the percep-

[17]It was probably one of these intermediates, and not the film proper' that was perceived by the patients of Gelb and Goldstein. See K. Goldstein und A. Gelb, Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter: IV. A. Gelb, Ueber den Wegfall der Wahrnehmung von Oberflächenfarben, *Zts. f. Psych.*, 84, 1920, 193-257.

[18]'Surface' is such a visual space-phenomenon as it has been worth while to name, and therefore to make into a formal, text-book 'perception'. If any of the other phenomena here noticed corresponded regularly with our modes of apprehension of the outside world; if, that is to say, veiled, or fogged, or immersed surfaces were part of our everyday experience; then they, too, would have been listed as definite 'perceptions'. They are, however, so uncommon that they have not been identified or stabilized by name.

tion is at the height of its complexity: we have the vestige of a surface, covered by a haze which is denser in its remote portion and becomes clearer as it extends nearer to us; and in the next following stage, that of the film proper, we have (with the disappearance of the last remnant of surface-color) no perceptive object at all, but a sensory datum, ultimate and unanalysable. If we persist in taking up toward this datum the perceptive attitude which we have maintained throughout the series, we are obliged to characterize the 'object' by negatives: it is not now localizable, it is not dimensional, it is not objective. There is, then a continuous increase of psychological complexity up to (and including) Group D, and then there is a sudden break, from 'perception' to 'sensation,' from highly complex to absolutely simple.

If we may trust the reports of the *Os*, the determining factor throughout this experiment is *localization*, and the phenomenological continuity of the series depends upon the steadily increasing difficulty of localization as cleancut surface is left and film is approached. The following reports are typical.

Bi (2 G) "When I get my fixation on the cloth, it flattens right out. Localization is definite. It's hard to keep it that way. It will cloud right up and be a disembodied green." Bi (3 G) "The color fogged up as I ran my eyes back, but as soon as I got definite localization there was nothing but surface, bidimensional and definitely localized." Bi (0 G) "The filminess and localization vary with fixation." Bi (3) "That film is determined as much by fixation as by the screens. I've got more or less at the same exposure from the same screen." H (0 P) "That thing comes so near to being nowhere that it's almost nothing." A (0 P) "I believe if I could definitely localize how far away it was, I could get it as a surface, and the thing works both ways." Br (9) "I can get a surface or a film either one, depending on where I focus." Br (3) "Film at first. Then surface. These are changes in convergence, I'm sure." H (1 G) "I can fixate that so that it becomes just a cloth surface. My eyes water and ache. When I do fixate it, it is localized fairly definitely behind the second screen, 15 or 20 cm." Bi (5 G) "Fixation would slip and film would form. Then I would get fixation and film would disappear."

Summary: Effect of Size of Hole

In reporting the results of this Series I, we have given the size of the hole in connection with every introspection quoted. There was much variation (both from *O* to *O*, and with the same *O* at different times) so far as the exact size of hole is concerned at which the various modes of appearance were reported; there was, on the other hand, great constancy in the descriptions.

Films were reported most frequently by all *Os* with holes 0 and 1 (2 and 4 cm. diam.). H and Br occasionally reported film with holes of other sizes, up to 14 cm. diam. The film-reports with the larger holes came in the early stages of practice.

Surfaces were most frequently reported by all *Os* for the largest opening (20 cm. diam.). With the cloths of very open weave, especially the green and orange cheesecloths, surface was often reported with smaller holes.

The *intermediates* showed a good deal of overlapping. The medians (all *O*s) were: soft surface, 18 cm.; veiled surface, 14 cm.; fogged surface, 10 cm.; immersed surface, 8 cm.

Series II. Effect of Localization

From the results of Series I it seemed probable that with our experimental arrangement the essential condition for the appearance of surface on the one hand or of film on the other is the localization or non-localization of the color quality. If this inference proves true, we have already answered half of our third question. We have found what changes the sheer color quality into the color of something. We should now be able, therefore, to change film colors to surface colors by giving the stimuli a definite localization. In Series II we attempted to test this hypothesis by bringing a strong localization-motive to bear upon what had been reported as a film color.

The colored stimuli used were Milton Bradley colored papers and the dark brown rep, because in the preceding series these had given pure film colors with larger holes than had any of the others. The most natural and obvious cue to localization was a fixation point in the center of the color-field viewed through the double screen.

We used both black and colored fixation points. They were cut with a beveled circular punch and were slightly more than 1 cm. in diam. The usual exposure time in this series was 5 sec. Longer and shorter times were tried without affecting the results, except when prolonged fixation was accompanied by staring. The following instruction was given. "After the usual 'Ready, Now' signal, a colored disc and a differently colored fixation spot will be exposed. Be sure to maintain fixation at the fixation spot. Report on the character of the spot and of the colored field in terms of localization, dimensionality, texture, and objectivity."

At first the *O*s had great difficulty in fixating the point, because the edge of the back screen offered rival fixation points. When they had learned to maintain fixation upon the spot, the color was almost always seen as surface. This procedure was possible for all except the 2 cm. hole, which did not permit of binocular fixation of the fixation spot.

A (2 BL, o)[19] "Behind the back screen several inches. Small round piece of orange paper placed on top of a piece of blue paper." M (1 D, bk) "Cloth with a black spot on it. Very definitely localized. Texture and weave very plain." H (3 D, bk) "If I attempt to fixate the color beside the spot, it is possible to make the color a surface in a plane with the spot." H (2 BL, o) "When I first looked at that, I was sure it was simply one bit of paper on another. Definitely surface behind the screen, opaque and

[19]The large letters after the figure in parentheses indicate the background; the small letters, the color of the fixation point. The backgrounds used in experiments from which these introspections are quoted were dark brown rep (D), and the following Milton Bradley colored papers: blue green (BG), dark blue (BL), and red (R). The fixation points were orange (o), yellow (y) and black (bk). Many other combinations were of course tried. The colors used did not affect the results at all.

solid." Br (2 R, bk) "A bit of red paper with the black stuck on it." Bi (1 R, bk) "I saw the spot pasted on in front. Both the purple and the spot were surface colors."

The spot and the color nearly always appeared as surface at first. Sometimes under steady fixation, even with the 5 sec. exposures, they took on a filmy appearance.

Br (2 R, y) "Yellow circle on a red background, which seemed at first to be surface; but as I looked, that seemed to be red air or space, not filmy enough for air. Momentarily it looked as if the yellow were in front of it, floating." H (2 BL, o) "Orange surface, bidimensional, definitely 15 cm. behind the screen. Definitely paper. The blue was simply some kind of blue surface. It might have been anything opaque and solid. As I stared at the orange spot, the blue became indefinitely localized, except that it dropped behind the orange."

In spite of these complications, we had shown conclusively that a visual cue to fixation was sufficient to change to surface an appearance which would otherwise have been filmy. This result in isolation would have been equivocal, however, because a bicolored film might perhaps be a visual impossibility. We therefore decided to see whether the cue to localization might not just as well come from some other sense department. The sense departments chosen for experiment were the tactual-auditory and the auditory.

In the lower part of each member of the double screen and also of the cardboard to which was pasted the colored stimulus were cut small holes 2.5 cm. square. A long rod passed freely through these holes. A piece of cardboard could be set up either against the color screen or in front of it in any desired position. *O* was allowed to feel through the screens with the rod until the tip of it touched the cardboard and then to tap, while looking at the colored field. Before the stimulus color was actually exposed, *E* put the pointer through the double screen in position for the tapping. Then the color was exposed for 5 sec., during which time *O* tapped the cardboard and looked at the color.

At first the *O*s found it impossible to connect the surface felt with the color seen.

A (2 BL, poking) "I don't think I'm realizing very well that I'm poking the identical thing. That's a hard thing to do. I'm looking at this thing up there and poking this thing down here, and there's no connection between them." Br (1 BL, poking color screen) "I can't think of those two as being the same. It was surface I was touching back there, but it didn't look and feel the same." M (2 BG, poking color screen) "The object poked seems to have no relation to the color." H (2 BL, poking in front of color screen) "Curious feeling of the unrelatedness of the two things. The things simply don't belong together. I certainly was not poking at the color; for the most part behind it. The color is not at the screen. It is unrelated to the screen, but doesn't seem very far beyond it. One would be perfectly willing to grant that it *might* be twenty feet beyond. For the most part I was punching behind where the color lay." Bi (2 BL, poking) "I don't see any point to sticking this pointer down there. I see color up here in this position and away off in some other universe I stick this thing through. I see the color and the color is nearer than the pointer."

After about six hours' practice, however, the color, which had appeared filmy, could be transformed to surface by the tapping, though it was rarely possible to hold the surface throughout the period of exposure.

Bi (2 BL, poking) "Sometimes while I was localizing the tip of the pointer, I seemed to see a surface." Bi (2 BL, poking) "If I turn my attention away from the color and think where that pointer is, I can see the color as blue paper." Br (1 BL, poking color screen) "At first they seemed quite incompatible, and then the blue receded backwards while I poked and became for a moment surface lying some distance behind the hole, but I couldn't continue seeing it that way." Br (1 BG, poking) "At one of the pokes, the color receded and became a flat surface and the tip of the pointer was touching the same surface." H (2 Bl, poking color screen) "If I attend very closely to the sensations (both tactual and auditory) that come from the punching, then the thing does change. I'm not sure just what happens. The color does get thrown back there and fairly definitely localized, pretty definitely bidimensional. It certainly becomes more like an object, though just what I couldn't say; more like a smooth, painted surface, fairly opaque." H (1 Bl, poking in front of color screen) "If I definitely focus at some distant point, although I can't see the point,—but yet I have this tactual cue that helps me to establish and hold it,—then the color seems to retreat and become very suggestive of a surface." H (1 BL, poking in front of color screen) "I then attended to the punching and tried to fixate a plane perpendicular to the end of the stick. At first the color was indefinite in localization but, as fixation steadied, there was a sudden shift of that color. It seemed to start to retreat, and then became very suggestive of a surface perfectly stable in localization for a second or two." A (2 BL, poking and told to concentrate attention on the poking) "It seems every now and then that I just poke a flat surface, and it looks like a flat surface." A (2 BL, poking) "It's hard for me to think that I'm poking the same thing that I see, but when I do get that realized, all of a sudden, just in a flash like that, it's a surface behind the screen."

In this experiment the cues to localization were both tactual and auditory. The *O*s were guided not only by sensations in the hand and arm, but also by sounds produced when the rod struck the cardboard. It therefore seemed advisable to see whether sound alone was an adequate cue to the change of film to surface.

Immediately after the color had been exposed, *E* tapped without jar, either on the back of the cardboard to which the color was pasted, or else on another piece of cardboard of the same kind, held in the hand before or behind the colored stimulus. The taps were mostly given in pairs, with an interval of 0.5 sec. between the two taps of each pair and an interval of 2 sec. between pairs. The length of the exposure varied with the number of taps given. The usual number was 5 pairs, requiring an exposure time of 15 sec. The total exposure time seemed to make no difference to the general results.

At first, again, the *O*s found it impossible to link up the sound and the color. The two experiences seemed to be disparate and unrelated.

Br (1 BG, tap 2)[20] "All I can say is that I couldn't bring the two ideas together. The color is much nearer than the sound. The hollowness of the sound gave an idea of solidity, which was not borne out by the filminess of the color. The sound was from cardboard, distinctly objective." A (2 BL, tap 10, in front of color screen) "I can't realize that it's the same thing. Seems as if you're tapping behind the color. Your tapping doesn't affect the color. I'm looking at the color, and you're tapping away off, but they have no connection for me. I can't get them connected." Br (1 BG, tap 2) "No realization. I saw the color as nearer than the sound." Bi (1 R, tap 10) "The auditory localization is very complicated. I'm afraid I'm paying too much attention to auditory localization and not realizing that the sound comes from the colored stimulus." H (1 BG, tap 1) "No change. I never localized the tap so far as fixation was concerned." H (1 BG, tap 10, in front of color screen) "I didn't succeed in bringing the tapping and the color into relation." A (1 BG, tap 10) "The color is in the focus of attention all the time, and I hear the tapping as an incidental noise."

Again, however, when the *O*s had had five or six hours' practice in these auditory localizations, the colors were changed from film to surface as they had been by the other two methods.

H (2 BL, tap 10) "It's hard to fix the distance of those taps and hard to fixate the point with the eyes. However, there was some shift over to the surface thing, *i. e.*, it became definitely localized 20 cm. behind the back screen, and definitely bidimensional." H (1 BL, tap 10) "It shifted to surface with the first pair of taps. That was maintained till between the second and third pair. Then I went over into a stare, and the thing became pretty positively filmy. After the third tap, I shifted back to fixation of the place where the taps came from and it became definitely surface with lights and shades,[21] and nearly papery." H (1 BL, tap 10) "I tried to fixate a point just behind the back screen, but this side of where the tapping took place. I maintained that till after the second pair of taps. It seemed to throw the color back a little, but otherwise it remained pretty good film. Then I fixated the point of tapping and got the surface thing." H (1 BG, tap 10, behind color screen) "The color tends to fluctuate between taps. The first pair of taps made no difference. With the second pair, the color definitely retreated. All of a sudden you find it back. You didn't see it go back. Bidimensional. About 20 cm. behind second screen. Certainly a surface, opaque and solid..... The transformation to surface always came with (or just following) the pair of taps." H (2 BL, tap 2) "After the second tap came a readjustment of fixation, and it became definitely surface." H (1 BG, tap 1) "It certainly does shift over to surface with steady fixation, when the eyes are set to fixate a point where the sound is localized. The shift came just after the tap, and then I tried to localize it in terms of the screen, but as soon as I tried to see where it was relative to the screen, it became cloud. Then when I returned to the fixation of the point tapped, it became surface again." Br (1 BG, tap 2) "Yes, they seemed to come from the same surface that time." A (1 BG, tap 10) "It seems that after I get fixed for realizing that back screen and I hear you strike it, I'm looking at the color and I'm conscious that it's there, but the

[20]The figure after the word "tap" indicates the number of times that *E* tapped during the exposure preceding the report quoted; thus "tap 2" means that a single pair of taps was given.

[21]There were of course no lights and shades; yet the appearance of surface occasionally led to their report. So one *O* reported the weave of a cloth, when in fact he was looking at a disc of colored paper. He had, it is right to say, been observing cloths during several preceding experiments; and the phenomenon was evidently an ordinary tied image.

tapping is in the focus of attention. But then, after you've stopped tapping, the color gets into the focus of attention again. In that shift I see the color as back behind the back screen, in two dimensions, just a flash of it. Then it goes back to what it was before." A (1 R, tap 10) "I got a flash of it receding from the back screen and being a perfectly flat surface right after you tapped." Bi (2 BL, tap 10, in front of the color screen) "Once I saw a surface when you tapped. Then again I didn't. Then you tapped again, and I did see a surface. Between taps the color was filmy." Bi (1 BG, tap 10) "For the most part the color was in front of the tapping. Just once I seemed to get a fair surface. I think it came partly by way of fixation." M (2 BL, tap 10) "The color changes from film to surface with each pair of taps, but returns to film between pairs."

Thus by giving localization through three different sense departments we had changed to surface a color that would otherwise have been filmy. In all these experiments, therefore, we are referred, as we were in Series I., to the predominating influence of localization. We have been able to change a film color into the surface color of a determinate object by introducing motives to localization, and the result has been in principle the same, whether the motive was visual, auditory-tactual, or auditory.

Series III. Observations of Familiar Objects

Series III was designed to answer the second question proposed in our introduction: "Can it be shown experimentally that the object-consciousness is sufficient of itself to ensure the perception of surface color?" If this were so, the knowledge that one is really looking at a familiar object should enable—nay, even compel—one to see surface, and prevent the reduction of surface to film.

Two forms of double screen were used, one of which was identical in essential details with that previously described, except that a single-sized opening of 9 cm. diam. was used. *O* sat at distances of 1.5, 1, and 0.5 m. from the front screen. No head rest was used. *O* could move the head and eyes freely. No instructions were given with regard to fixation. Most of the *O*s found it easiest to fixate on the edge of the hole in the back screen.[22]

The stimuli were familiar objects: a board, a fur neck-piece, a strip of felt, a satin cushion, a woolen sweater, aprons, coats, and cloaks. *E* placed the stimulus in such a position that it entirely filled the hole, and also projected above the upper edge of the double screen. In direct vision *O* could not have seen the stimulus above the screen and in the hole at the same time; but by slight movements of the head and eyes it was possible to make swift comparisons of the two. Under these conditions, the *O*s never described the color in the hole as ordinary surface color, except when they also reported some detail of structure which offered a fixation point.

[22] Katz, *op. cit.*, 38.

The other form of double screen was used only in this series. It was designed to compel parallel vision.

Like the regular form, it was made of grey cardboard fastened to wooden frames. In each screen were cut two holes of 1.5 cm. diam. The wooden frames carrying the cardboard screens were then clamped at the front of a table, and *O* sat as close as possible and looked directly into the front screen, as into a binocular telescope. Since the interocular distance differed slightly for different *O*s, different double screens constructed on the same principle were used, and the precise distances between the holes were chosen empirically to suit the *O*s.

The stimuli were small familiar objects: soap, chocolate, paper, and the like. The stimulus was first handed to *O*, and he was allowed to examine it as he pleased. Then *E* placed it behind the double screen. *O* did not know how far from the double screen the object was to be placed. With this exception, the *O*s worked with full knowledge of the apparatus and manipulations. They therefore had "the consciousness of an object upon which the colors arise," which Katz regards as the essential condition for perceiving surface. Yet, under the above conditions of experimentation, surface colors were never reported.

III. Bulk to Film

Series IV. Episcotister Before Surface With Fixation Point

We planned Series IV as a direct parallel to Series I. We intended, that is, to begin with a positively bulky color, and to carry this over by intermediates, phenomenological or psychological as the case might be, to film color. Bulky colors were secured without the use of actually bulky stimuli by means of colored episcotisters.[23] When an object is seen through a rapidly rotating episcotister, it appears as if seen through a haze of color. Under certain conditions, this haze expands to a positive bulk.

The episcotister used in these experiments was made in two identical parts, each of which was constructed from a disc of thin but very stiff cardboard, 51 cm. in diam., by cutting out 6 sectors of 30° each, leaving 6 equally spaced sectors of 30° each. These were covered on the one side with blue paper and on the other with reddish orange paper. These two colors were chosen for convenience. Any other colors might have been used. In cutting out the sectors a cardboard margin, 2 cm. wide, was left on the outer edge, and a cardboard disc, 12.5 cm. in diam. at the center for support. The episcotister was mounted on an electric motor, operated by pedal. By proper combination of the two pieces, it was easy to use any amount of color from 180°, which gave a slight, vague, barely perceptible haze in front of the background, to 360°, which of course was no longer an episcotister, but a solid color disk. It was possible to use either pure blue or pure red-orange or any desired mixture of the two colors, by using the blue side of the one disc and the red-orange side of the other. Other differences in color could be obtained by varying the color of the background. The backgrounds used were whole sheets of Milton Bradley colored papers tacked to the wall of the dark room. The table, carrying the double-screen and the episcotister, was so placed that the back screen was 65 cm. from

[23] *Ibid.*, 310.

the colored background. The episcotister could be moved to any desired position between the screen and the background. The exact position seemed, so far as our results were concerned, to make no difference. Distances actually used were 10, 15, 20, 25, 30, 35, 40, 45, and 50 cm. from the back screen. Even with the two extremes, no consistent difference in reports could be discovered. As one *O* remarked: "I don't seem to get any variation in the experience, however you move things behind." In the latter part of the series, the distance between the episcotister and the back screen was kept at 20 cm.

The *O*s were given typewritten instructions, similar to those used in preceding series. The exposure time was 5 sec.

Under these conditions, with screen 3 (8 cm. hole) appearances precisely similar to those of Groups B, C, D of Series I were secured,[24] an increase in the amount of color in the episcotister having precisely the same effect as the substitution of smaller holes had had in the series without fixation points. A further increase in color, however, did not result in the immediate appearance of film, as it should have done on the analogy of Series I. Two other stages, which we have designated as e and f respectively, were found. In Group e, the last vestige of surface is gone, and the fixation point, whose contours are by this time badly blurred, is seen floating deep in a thick substantial mist.

Bi (3 O, bk, 336 O)[25] "The film is well this side of the fixation point and definitely behind the screen. It hints at dimensionality. The surface has been out for quite a while. I didn't realize it was gone." Bi (3 O, bk, 330 O) "Now the fixation point is away back in a dense tridimensional orange. It might be daubed on the back of it for all I could tell, only the film wouldn't support an object." H (3 O, bk, 240 O) "Uncertain localization. Curiously, the spot seems to be behind the screen. The color might be this side of the spot. The color seemed more indefinite than the spot. Very fuzzy, felty, soft. No particular objective reference. Cloudy. Translucency is suggested." H (3 O, bk, 330 O) "The spot always seems to be back in the color, like something indistinctly seen through haze. The haze is simply out there, and that is all you can say. The haze is around the spot and in front at the same time." Br (3 O, bk, 330 O) "Fairly dense haze. The fixation point is floating somewhere in the haze. The position of the whole thing seems moved toward the hole, but not as far back as when seen on the surface." Br (3 O, bk, 336 O) "The haze was still denser. The fixation point came and went. Very indistinct. It might be located anywhere within the haze." Br (3 BL, bk, 270 BL) "The dense haze fills all the space up to the hole. The spot is somewhere behind that blue haze or immersed in it, so that you can see the blue in front of it." A (3 BL, bk, 342 BL) "Dark blue mist. I could see the fixation point through it a good way back. I couldn't see any surface." A (3 O, bk, 339 O) "I looked into an orange mist that came up to the back screen, and far back in the mist I could see the fixation point very vaguely. I don't think the mist looked as if it went farther back than the fixation point, but I'm not certain." A (3 O, bk, 348 O) "Looking into orange mist. It came up to the

[24] For facility in cross comparison, we shall continue to refer to these by the same letters as in Series I, but shall use small letters instead of capitals, so that the two series may be kept distinct.

[25] The second number in the parentheses indicates the number of degrees of color in the episcotister; the letter following it, the color used on the episcotister.

back screen, and I don't know how far it extended; not very far. Fiery kind of mist. It looked as if it might have been made up of thousands of little particles. I could make out the fixation point, somewhere in the mist. Its outline wasn't distinct." M (3 BL, bk, 210 O) "Fixation point barely visible through a dull pink tridimensional haze. The fixation point is indefinitely objective and bidimensional."

In Group f, the fixation point is no longer an object, localized with difficulty in the midst of a thick haze. It has become a film, as positively indefinite in localization as the color around it, which approximates the films obtained by the reduction of surfaces, the only difference being that it is slightly more suggestive of tridimensionality.

H (3 BL, bk, 180 BL) "At first glance it's the old indefinite cloudy thing. Then I got my fixation point, but couldn't hold it. There was a good deal of fluctuation. Localization was not very certain, but there was a tendency to put it behind the back screen. Bidimensional, but might easily have been tridimensional. No objective reference. Strong hint of translucency, but yet I couldn't actually say that I saw into it; more of the cloudlike effect. The fixation point shared the character of the color." H (3 BL, bk, 300 BL) "I saw the fixation point dimly all the time. No difference between the spot and the blue. Indefinite localization. Strong hint of tridimensionality. Strong hint of translucency, and yet I couldn't see into it. For the most part I haven't been able to focus on the fixation point." Br (3 O, bk, 330 O) "I saw the fixation point as a very faint grey filmy spot, not flat. It had a film in front." Br (3 O, bk, 342 O) "Film, perhaps a little denser. I see the fixation point as a grey nebulous mass in the center." Br (3 O, bk, 348 O) "Very dense film. Slight greyishness in the center. It didn't have any definite outlines, and didn't appear like a fixation point at all. Just a bit of grey film." A (3 O, bk, 339 O) "I could see the fixation point very vaguely. I don't think the mist went back any farther than the fixation point, but I'm not certain. I can't say whether the fixation point is standing out in the mist or whether it's behind it." M (3 BL, bk, 180 O & 150 BL) "The fixation point is barely visible in a mass of red fog, which is tridimensional. The fixation point is filmy, indefinitely localized and non-objective." Bi (6 BL, bk, 324 BL) "Fixation point is back again just a little. I can make it out with some certainty now, just a little dark spot. It's filmy, too." Bi (6 BL, bk, 327 BL) "Able to see the fixation point about a third of the time. It's also filmy." Bi (3 BL, bk, 180 O & 30 BL) "Fixation point and fixation screen are both films now, but not very far removed from surface. All the color is close together in a thick plane, no texture."

When the amount of color in the episcotister was increased so much that the fixation point was no longer visible, the resulting appearance was a film, practically identical with the films found in Series I. One *O* called attention to this fact.

Bi (6 BL, bk, 330 BL) "Fixation point gone. It's a great film, filling up the whole area behind the cardboard screen. As far as I can tell it doesn't look any different from the plane colors of the other series."

Here, as in Series I, there were of course a few equivocal cases, which seemed to fall midway between two of our groups. The following may serve as examples of a suggested intermediate between e and f.

Br (3 O, bk, 180 BL & 150 O) "Pink film. The fixation point floats in the film. Fairly dense. Pinkish right up to the hole. The spot is filmy, but seems to lie right back in the film." H (3 BL, bk, 180 BL) "The fixa-

tion point was definitely localized at 30 cm. or more behind the screen, probably bidimensional. No objective reference. Opaque, solid. The color was different. I know little about the color. Not definitely localized except behind screen. So far as I can say, it was bidimensional. I don't know that I saw it dimensionally at all. No objective reference. Opaque; whether solid or not I don't know."

The dependence of all these appearances upon fixation is illustrated by many reports, of which the following may serve as examples.

A (3 BL, bk, 306 O) "The fixation point is back. I can't see it on a surface. When I see the fixation point, the mist extends lots farther back than when I don't see it." A (3 BL, bk, 312 O, immediately following the previous report) "At first I looked into orange mist, which had a little tinge of lavender, extending back an inch or so. Then I looked carefully and saw the fixation point, and the mist spread away back." Br (3 BL, bk, 300 BL) "No fixation point visible, just dense blue film. I can't see through it as far as when I looked through it to the fixation point." Br (3 BL, bk, 321 BL) "When my fixation comes front, the color becomes very dense and occupies all the space between my fixation and the screen; when I maintain fixation, I get what I described last time, *i. e.*, approximate surface behind at the fixation point, with dilute film out in front, which came toward the screen, but not to it. The surface was bidimensional; the rest, tridimensional." Bi (3 BL, bk, 324 BL) "No fixation point visible. I could make out an approximate surface back where the fixation point would have been if I could have seen it. The rest was thick haze, uncertainly localized." H (3 O, bk, 336 O) "If I look simply at the color I get it indefinitely localized, bidimensional, hinting at tridimensionality; sort of a rudimentary tridimensionality. No objective reference. Pretty much the cloud. A little hard, maybe. Hints at translucency. The spot is simpler a darker bit in the color. If I look for the spot, then I seem to be able to throw the spot back."

A short supplementary series was tried in which the background, instead of being a plain sheet of colored paper, was a sheet of cardboard bearing the printed symbols PSYCHOLOGY I B and a rough drawing of an arrow. Screen opening no. 3 was used, and the amount of color in the episcotister was varied from 180° to 330° by steps of 30°. The resulting appearances were surfaces obscured by hazy or bulky colors, which varied in depth and density according to the amount of color in the episcotister. The more color in the episcotister, the denser of course was the haze. The apparent depth or degree of tridimensionality increased up to about 270° and then diminished as more and more color was added. When the letters could no longer be seen the resulting appearance was the familiar film.

Series V. Episcotister Before Surface Without Fixation Point

Series V was undertaken as a forlorn hope to see whether our practised *O*s might possibly report the impression of bulky color, as in stage e of the preceding series, even though the fixation point had been removed. The arrangement, therefore, was identical with that of Series IV except that the colored backgrounds were uniform over their entire surface.

The attempt was partially successful. Usually, it is true, the reported color was almost identical with the films obtained from the reduction of surface colors; like other films, it was non-objective, unlocalized, and pretextural; but it often seemed to hint more strongly at tridimensionality; and sometimes tridimensionality of uncertain amount was positively ascribed to it.

Br (3 O, 180 O) "Red bulk rather than red film, like a red liquid." Br (3 O, 180 O) "Dense red film, redder than before. It's almost as if I were looking through a red light or into a brilliant flame. Luminosity." H (6 O, 180 O) "Too luminous for cloudy; too much like a glowing ember where you only see the glow. No other perceptive motive there. It tends toward a glassy or liquid bulk, though it isn't that." Bi (3 O, 270 O) "Thicker now and more nearly tridimensional than before." A (3 BL, 180 BL) "It comes up to the back screen and seems to come out just a little bit at the middle; just floats out like mist." H (3 O, 180 O) "Suggestion of tridimensionality. Hint of depth. Sometimes part of the field was almost bulky." H (3 O, 279 BL) "For the most part behind the screen, but not very definitely localized. Tridimensional for the most part. Translucent, like bulky liquid tending slightly toward gaseousness. It definitely had depth." M (3 O, 180 O) "Very dense haze. Comes up to the back screen and extends back a short distance." M (3 BL, 330 BL) "Stationary blue haze, doubtfully tridimensional, suggestion of translucency. Perhaps lighter in front and dark behind. Indefinitely localized."

Associated with the tendency toward bulkiness, the *Os* sometimes reported a sort of incomplete objectivity.

H (3 O, 180 BL) "For the most part tridimensional, although the tridimensionality increased and decreased, with a wavelike effect. Objective reference, but not specific. It didn't come as an object, but you could make it into an objective something."

Thus in Series V we seem to have found something like true psychological intermediates between film and bulk. Equivocal appearances tending more or less toward tridimensionality were reported from time to time throughout the series. Whereas, under conditions of binocular vision, film changes into surface at a single step by way of superadded localization, film seems to pass into bulk by insensible degrees, as if by mere extension of that suggested but unrealized tridimensionality which all *Os*, those of Katz as well as our own, have remarked.[26] We believe, nevertheless, that *these intermediates are still nothing more than phenomenological.* It must be remembered that the bulk in our bulk-to-film series was itself only generically objective, an affair of haze or mist in general, and not specifically objective like the surface color of paper or cloth; nor was the background so marked or shaped as to suggest a determinate object seen through an intervening haze; it was mere expanse of color with a central fixation point. Even in stage e, *O* may report entire lack of objective reference, especially at an early stage of training. Hence it is probable that we are here in presence of perceptive formations whose likeness to any common object of percep-

[26]Katz, *op. cit.*, 7.

tion is markedly less than that in the formations named b, c, d. This conclusion is borne out by reports of vague or general objectivity, reports which increase in frequency as the training of the *O*s progresses.

For the reader's convenience we insert here a brief table of the phenomenological results of our two main Series.

SERIES I		SERIES IV
A	Soft surface	
B	Veiled surface	b
C	Fogged surface	c
D	Immersed surface	d
	Substantial haze	e
	Cloudy haze	f
	FILM	

A, B = b, C = c, D = d, and e are without doubt perceptive formations. We believe, as has been said above, that f is also psychologically a perception; in spite of the unfamiliarity of colored hazes in everyday life, the *O*s tended more and more, as observations went on, to report a vaguely objective reference. The fact, however, that any degree of doubt or hesitation can arise at this point supports our conclusion that the ultimate film is more suggestive of tridimensionality, less positively bidimensional, than Katz has made it.

In connection with these bulky hazes, a rather curious phenomenon seems worthy of remark. Some of the *O*s, most frequently Br, reported not only positive tridimensionality, but also differences of hue, between the front and back regions of the haze.[27]

Br (6 R, 300 BL) "Dense blue film with the faintest tint of purplishness in the front and dense blue toward the back." Br (6 R, 270 BL) "Deep purple, pinkish toward the front..... In all these the film begins at the screen and goes back from there. Usually the front is filled in with a lighter film, misty." Br (3 YG, 180 BL) "Greenish film, whitish-yellowish at the front and green farther back. It starts at the back of the back screen and goes back a long distance, almost to infinite distance. It's the sort of haze that I've seen heaps and heaps of times at sea."

One of the byproducts of Series IV and V was the discovery that bulky films could be produced with somewhat larger holes than the films of Series I. The difference made by the use of the episcotister is well illustrated by the following pair of reports, taken successively in the same observation hour. The first was taken without episcotister in front of color, but with motor running, so that there was no auditory cue to the difference. The second was the same as the first, except than an episcotister with 270° of orange was between the color and the back screen.

[27] It will be remembered that the *O*s were not instructed to report upon hue. Hence these observations were spontaneous. It is possible that the other *O*s might occasionally have noted these differences, if the instructions had called for them.

H (6 O) "Quite definitely localized, bidimensional, fairly definite objective reference, in so far as it was a smooth surface (paper or anything of the kind). Solid, opaque." H (6 O, 270 O) "Very indefinitely localized. I don't know whether I can even say it was back behind the back screen. Bidimensional, I guess. There were moments when it seemed almost tridimensional. Hint of translucency. The term 'cloudy' is not adequate, 'filmy' is not adequate. It's just a bit of luminous color."

Series VI. Control Series

The best guarantee of the validity of experimental results is agreement among a number of trained *O*s. Our quotations have shown that there was in fact a very high degree of agreement. Series VI was planned as a further test of reliability. To see whether the use of the episcotister carried with it any imaginal suggestion which might have exaggerated the differences between the film colors of Series I and the bulky colors of Series IV, we turned the episcotister in such a way that it did not come in front of the colored background at all. The episcotister was left on the motor, so that it might run at the same speed and produce the same sound. The shadow of the episcotister fell on the wall some distance from the colored paper, so as to have no effect on its appearance. Since the colored backgrounds were tacked to the wall, they could not be jarred or in any way affected by the motor on the table. Hence it made no objective difference in the color whether the motor was running or not. The only difference between exposures during which the motor was running and those for which it was turned off was the sound. It was assumed that the sound might act as a suggestion to the *O*s that they were seeing the same sort of thing as in Series IV and V. Between exposures *E* moved the motor about so as to suggest, by sound again, that the episcotister was being put in front of the color or removed. No verbal suggestion was given.

The screen openings used were 3 and 0 (8 and 2 cm. respectively): screen 3 because it was that most frequently used in the series to be tested, and screen 0 because it had invariably given film in Series I. With *O*s A and Br screen 6 (14 cm.) was also used for purposes of comparison.

In every case the *O*s immediately or almost immediately discovered that the two experiences were identical. Br and A were absolutely unaffected by the suggestion. H, as was shown by the cautious and hesitating nature of his reports, was apparently affected by the suggestion in the first two observations, but in the third he settled down to objective report, and thereafter found no difference between the appearance of the color when the motor was running and when it was not. Fifteen subsequent trials failed to show any the least trace of suggestion. Similarly, Bi showed a possible slight effect of the suggestion in the first three trials; but even here the differences reported were scarcely

greater than those occurring in the course of a normal series; and his reports in 15 subsequent trials were entirely free from evidences of suggestion. This record affords objective proof of the reliability of the *O*s.

IV. The Monocular Film

Series VII. Holes of Various Sizes

In this series the apparatus and procedure were identical with those of Series I, except that, instead of making all observations binocularly, the *O*s were asked to alternate binocular and monocular observations and to compare the two.[28] The reports of all *O*s showed that with monocular observations size of hole makes less difference than with binocular. In every case the color appears as an almost bidimensional film, which tends to lie in the same plane as the back screen. In having less suggestion of tridimensionality about it than the binocular films of previous series, these monocular films are closer to the typical film as Katz conceives it. The following comparative observations illustrate this point.

H (°, l) [29] "With one eye the two holes seem very much in the same plane..... The color was just color. Non-objective and unlocalized. It was certainly behind the cardboards, but I couldn't tell more than that. It tended to come up close against the card. Dull, opaque, fuzzy, soft. Slightly suggestive of translucency; not much. Neither bidimensional nor tridimensional. Bidimensional spread is clear enough, but there is nothing definite about the third dimension. Tridimensionality is suggested rather in the softness, the fuzziness, the sponginess of the thing than in anything else." H (°, r) "Little difference between the two eyes. The texture was perhaps a little less fuzzy and soft, but still suggested the third dimension." H (°, b) "Funny how that changes! With two eyes, there is a definite space between the screens. The second was definitely 20 cm. behind the first. The color was still non-objective, localized at or behind the second screen. More certainly tridimensional, though not in any determinable amount. The texture was more translucent, more motile, as though there were liveness in the color. I think that's probably what we have called luminosity. More dense, glass-like; not clear, transparent glass, but a little more solid." Bi (°, r) "Equally filmy, but pretty much up to the hole. I tried to force my eye back in it, and it didn't work somehow. It left me with something that was not deep in the third dimension, but not surface." Bi (°, l) "I have a feeling that I ought to be able to force that film to be deep, but I don't believe I can. I can pierce it a bit, but it stays pretty much a flat, dense film at the hole." M (°, b) "Indefinitely localized, except somewhere behind the back screen. Non-objective. Soft, loose, penetrable texture. Dimensionality uncertain, but tridimensionality is at least strongly suggested." M (°, r) "Non-objective, just a patch of color in the hole. Soft, thin, filmy. Probably bidimensional, though here (*i. e.*, in monocular observations), where differences of localization are not

[28] We took our cue for this monocular series from Katz' suggestion that monocular observation of ordinary objects affords a mode of appearance intermediate between film and surface: *op. cit.*, 9.

[29] Small letters after the comma in the parentheses indicate the mode of observation. Thus l means "with the left eye;" r, "with the right eye;" and b, "with both eyes."

so noticeable, dimensionality also seems equivocal. All things seem shoved up more nearly into the same plane. The front screen, the back screen, and the color all lie, or at least look as if they might lie, in the same plane." Br (2, b) "Soft, filmy brown medium. Localized near the hole and yet it seems to go back a bit from that. Dense, cloudy appearance." Br (2, r) "Same, except that it's like a plane, even, fine-textured cloth at the hole; really a film, but I can imagine a cloth like that; one of those soft georgettes." Bi (2, b) "I have a suspicion that there's a deeper film with two eyes than I saw with a single eye. The one with both eyes is like a fog; the one with one eye is like a rotating disc." Bi (2, r) "I looked for the deep character that I thought I saw before. It is not there with the single eye, though what I saw was certainly not a surface. It still had a filmy character." A (2, b) "I see a brown misty thing up in the hole and behind it, too." A (2, r) "I can't say whether it looked up at the screen or behind, but it wasn't misty-looking as it was when I saw it with two eyes. More surfacey. Not a slick surface. More like blotting paper."

Thus it is evident that monocular observations with the small holes gave a flatter, more nearly bidimensional color than did the binocular observations.

With the larger holes, the reverse proved true. Although the object could now be easily identified, it did not appear to have so sharply defined a surface as in the binocular observations.

M (9, b) "Ordinary cloth, definitely localized behind the screen." M (9, r) "Positively cloth, but greatly softened, and not a sharply defined surface. Velvety. Not much space between fore and back screens or between back screen and cloth. All are in nearly the same plane. Less certainty of localization than when viewed binocularly; or rather, less positiveness of difference in localization. The very question of localization seems out of place in these monocular observations." H (9, r) "Little difference monocularly between this and the last (hole no. 2, 6 cm.). Opaque, softened, a little more certainly behind the screen when I use the side of the hole as a reference point, but not when I look straight at it." H (9, b) "Fairly sharp surface. No hint of softness. Certainly a colored something. Cloth, or it might be a colored wall. Localized very definitely behind the second screen at a distance of 20 cm., more rather than less. No suggestion of depth, just an opaque, solid substance." Bi (8, b) "I get a film that goes all the way back to the background, but I can nearly see the cloth, if it is cloth. The cloth was away back by the wall. The haze was everywhere between the cloth and the hole. Part of the time it seemed equally dense throughout, and part of the time it seemed thinner up in front and denser behind." Bi (8, r) "Dense film. Thick plane at the screen. It huddles up to the front. It stays up. I can't force my gaze back through it." Br (8, b) "Surface, some distance behind second hole. Flat piece of cloth." Br (8, r) "Film, which is difficult to distinguish as film, because it seems that flat plane, lying on the back hole." A (9, b) "Back behind screen, 8 or 9 in., and now it looks with two eyes very much as it did with one a while ago, although it has more quickness and liveness." A (9, l) "Right up in the screen, and a little duller than before. By dull I mean that it hasn't that quick, lively look."

From these and similar observations it is evident that, with monocular observations, the largest holes like the smallest gave an appearance of filmy texture. In fact one *O* (H) remarked: "The one-eye appearance is nearly uniform for the whole range of sizes."

For the sake of completeness we append a few typical observations taken with intermediate sizes of holes.

Br (4, b) "More filmy and behind the back hole. I couldn't say whether it was really bulky. Indefinitely localized. No definite front surface." Br (4, l) "It looked a little more flat and seemed more a plane, but not as definitely a plane as when I saw it binocularly through those large holes." Br (4, r) "Same. Filmy, soft, fuzzy appearance, but not so thick as when I saw it with both eyes." Br (6, b) "Filmy, thick, dense, foggy." Br (6, r) "Flat, even, filmy thing; really like a spectral color hung in space." Br (6, l) "Same. More plane and even than with both eyes." M (4, b) "Brown surface, seen dimly and localized with difficulty behind a brown medium of increasing density which lies somewhere behind the back screen. The surface is cloth." M (4, r) "Brown cloth, but very soft, thin, and filmy. The brown might be at, or even slightly in front of, the screen. Certainly there is not much if any difference of distance between the screen and the color, or between the two screens; but the screens themselves are not localized with any degree of precision. Not so much suggestion of tridimensionality as with two eyes, but the surface character is not positive either."

Without stopping to multiply illustrations we may therefore say that, throughout our whole range of holes, including those that were large enough to permit a recognition of the object, monocular observations gave appearances which in texture, dimensionality, and indefinite localization approximated fairly closely to the Katz film.

Series VIII. Monocular Observations with Episcotister

Obviously the next step was to find out what happens to bulky colors under monocular observation. For this purpose we used an experimental arrangement identical with that of Series IV. In the episcotister we used dark blue, orange, and various combinations of the two. The background was a sheet of dark blue paper with a black fixation spot in the middle. As in Series VII, monocular and binocular observations were alternated for purposes of comparison. It was found that, whereas binocular observations with a suitable amount of color in the episcotister gave good bulky color, monocular observation reduced this to a filmy appearance.

H (3 BL, bk, 180 BL, b) "The spot was definitely 25 or 30 cm. behind the back screen. It tended to give considerable depth to the color. The color was transparent, dense, glassy, flinty. It was still non-objective. I don't know whether the color filled all the space between the screen and the dot, but it tended to have that meaning." H (3 BL, bk, 180 BL, r) "More nearly flat in form. Non-objective, but it suggested some kind of soft surface. Quite opaque. No translucency or transparency. The color was localized back at the dot. I don't think the color or the dot either was quite as definitely localized as the dot was before (*i. e.*, binocularly), but the color was with the dot." H (3 BL, bk, 180 O, b) "The fixation point was definitely 25 or 30 cm. behind the back screen. The color itself was certainly non-objective and pretty definitely tridimensional; a mass, almost a body of it, extending from the region of the screen back to the dot. I couldn't be sure whether it extended clear to the dot or not. Translucent, soft, not much glassy." H (3 BL, bk, 180 O, r) "The color and dot

seemed to lie together and tended to be localized nearer the screen, but there was not absolute definiteness of localization. Less massiveness to the whole experience, but yet not a definite surface with objective reference. Soft, fuzzy, suggestive of tridimensionality but not in the same sense. The whole thing is dead, static, more like a soft surface. No suggestion of translucency or transparency." A (3 BL, bk, 270 BL, b) "I saw tridimensionality that time. Thick fog came up to the hole. I could see into it. I couldn't tell how far it extended. The fixation point was in it somewhere." A (BL, bk, 270 BL, r) "Bidimensional, as if I were looking at something that let the light through; but I saw it in two dimensions. Like thick colored glass that's soft-looking, but you don't see anything but the front of that glass; like glass in church windows. If you didn't know it was glass you'd hardly call it glass. Not transparent, but translucent." A (3 BL, bk, 270 BL, b) "I couldn't help seeing it as tridimensional. I tried to hold it in the same place as with one eye, but I couldn't. The two things don't look anything alike." Br (3 BL, bk, 210 BL, b) "Bulky, dense medium. The fixation point was buried in it somewhere." Br (3 BL, bk, 210 BL, r) "Flatness, filminess, rather than surface, and yet it's all in one plane, or almost all in one plane. With two eyes you seem to be looking into something almost glassy-like. This is like the blue of the sky. This has the appearance of being penetrable, but you don't have that impression of seeing space that you have with the other." Bi (3 BL, bk, 180 BL, b) "Hazy smokiness which goes clear back to the fixation point. It's open and loose. I didn't see it come clear up to the screen I didn't see it fall short either." Bi (3 BL, bk, 180 BL, l) "This tended with one eye to be flat. The fixation point tended to come up to the screen. Anyway they got crowded together." M (3 BL, bk, 270 BL, b) "Fixation point badly blurred and localized with difficulty in a dense, semi-transparent fog, which began at or near the back screen and went back an uncertain distance." M (3 BL, bk, 270 BL, r) "Color and fixation point are both filmy, and seem to lie in approximately the same plane as the screens. Both screens seem to lie in very nearly the same plane. No part of the total experience is localized with any degree of precision. The color appears soft but not actually penetrable. It is non-objective."

Hence it is evident that all the color appearances which we have here studied, when viewed monocularly, take on a more or less filmy appearance. They become indefinitely localized, pretextural, predimensional; and except where familiar configuration carries with it the meaning of a familiar object, as in the case of the screens themselves or the brown cloth as seen through the largest holes, they are also non-objective.

V. Conclusions

1. Our experimental results indicate that the simple visual datum is a "film," predimensional but tending more strongly toward tridimensionality than Katz' bidimensional *Fläche*. The film is also pretextural, non-localizable, and non-objective. It has an attributive character of spatial spread or diffusion, to particularize which we must have recourse to qualified perceptive analogy.

2. There are no psychological intermediates between film and surface. Our results show a sharp break between the true film and the immersed surface, the haziest of the surfaces, a

break which involves a complete change of attitude. The localization of the color in the case of the surface is the determining factor in the shift from film to surface.

3. Between film and bulky color the shift of attitude is more tentative and cautious, and the transition is in a sense more gradual. There are, nevertheless, no true psychological intermediates.

4. There is a wealth of phenomenological intermediates, any one of which might have been stabilized as a perception if our everyday experience had required such stabilization.[30]

[30]We make no claim to have reviewed the whole series of intermediates. It is possible that another approach to the general problem, and in particular the bringing to bear upon the film of strong perceptive motives other than that of localization, might lead to another classification and might reveal new phenomenological modes. We can say, however, that in a good deal of casual experimenting, such as naturally suggested itself in the course of an investigation like the present, we have found no 'leads' toward a further variety of visual appearance at the level of our enquiry.

CAN THE PSYCHOPHYSICAL EXPERIMENT RECONCILE INTROSPECTIONISTS AND OBJECTIVISTS?

By J. R. KANTOR, University of Indiana

In the following article the writer assumes that it is exceedingly desirable (in order to establish psychology as a science and to promote necessary mutual understanding) that psychologists should agree upon the fundamental data of their investigative domain. Accordingly, he undertakes to point out that such an agreement not only appears possible but that there is in fact more agreement between psychologists of various schools than really appears on the surface. In fact the writer believes that he has found a possible basis for an almost complete agreement between introspectionists and objectivists in some recent discussion of the stimulus-error and the psychophysical experiment.[1] In addition the writer attempts to characterize what to him appear to be the conditions underlying the present dissension among psychologists and to propose some suggestions for overcoming such disharmony.

I. Why Agreement among Psychologists seems Necessary. Are not psychologists after all interested in the same series of natural events? If this question is affirmatively answered then agreement there must always be between psychologists no matter what their presuppositions are, provided that they exercise meticulous care to keep close to the facts within their special province of investigation.

Can it be to the advantage of psychology if the several psychologists indulge in such violent disagreements that they can question the value of each other's work and its scientific validity? And yet to the existence of such derogatory ideas of each other's work on the part of psychologists the content of our psychological literature could hardly bear more persuasive testimony. Surely a more united background for work than this seems to indicate is one of the first requirements for the advancement of any science. As a matter of fact the indispensable harmony among psychologists is supplied by the omnipresent peacemaker in two ways. In the first place, he tells us that after all there is no quarrel because the opposing parties merely study different aspects of the same actually existing data. The grain of truth in this contention is found precisely in the

[1] E. G. Boring, The Stimulus-Error, this JOURNAL, 1921, 32, 449-471.

fact that if our opposing psychologists are dealing with facts at all there is really only one set of such facts. But as in the case of most intercessors there is no profound examination of the difficulty involved between contending parties. Thus the problem is not considered (1) whether the psychological data have in fact the alleged aspects, or (2) whether they can be separated and separately studied. Now in order that anything in the field of science should be a datum it must be a unit object or occurrence, and so we assume that the mediators to whom we refer consider the field of psychology to involve different kinds of facts. Indeed it would be strange if such a large field as that of psychology did not contain many different types of facts even if all individual occurrences constituted the same form of datum. As a matter of fact, it is possible for different psychologists to choose, for their particular study, reactions which differ from each other as thinking and reflexes. But are such basic disagreements as we find in psychology today merely due to the fact that psychologists are dealing with one or the other type of responses? No, the quarrels concern the nature of the data themselves.

The mediators, in the second place, claim that the violent disagreements in psychology do not much matter since introspectionism and behaviorism, let us say, are different hypotheses concerning the data of the psychologist's domain, and it is not so very important, they say, that psychologists should agree upon their interpretations. Such disagreements (they further say) are found in physics, that most stable of sciences, in which light, for example, is sometimes interpreted in corpuscular and sometimes in radiational terms. Here again we must point out that the opposition would not long survive if the opponents were not inquiring into the nature of their data rather than offering an interpretation of them. Consequently an entirely different condition prevails in psychology from that in physics, in which science no question is entertained concerning the "data" of light, for example.

Now, as we have indicated, such marked differences of opinion as today exist in the domain of psychology are hardly conducive to anything but futile argument. So far indeed is such violent dissension from promoting any real comprehension of psychological facts that we ought not to take lightly the existence of such extreme views as those of the different varieties of introspectionism and behaviorism. Rather we should attempt to discover what in the facts of psychology themselves argues for agreement concerning the data, no matter how differently such data might be later interpreted.

II. Evidences of Possible Agreement in Psychology. Signs are many and important too that much general agreement

is possible and in fact gradually being arrived at with respect to the data and fundamental principles of psychology. Tremendously encouraging is the discovery that students of psychological phenomena, although starting from different theoretical presuppositions, when bent upon a serious investigation of actual facts, can come to much the same conclusions concerning the character of psychological data.

Now it so happens that the original psychological experiment, namely the study of the psychophysical response, is capable of affording us considerable insight into the nature of a psychological datum. The psychophysical experiment is, in the first place, a fairly simple form of psychological reaction which has been thoroughly and frequently discussed; consequently we should experience no difficulty in studying the crude facts with the hope of arriving at some fundamental psychological agreement. For we have full faith that if we can only get down to our crude data with a complete freedom from philosophic bias[2] an effective *rapprochement* between psychologists will not only be possible but to a certain extent inevitable.

The second reason for hoping to find a basis for the reconciliation of psychological opposites by a study of the psychophysical experiment lies in the fact that a genuine attempt to come to an agreement is already to be found in Boring's recent discussion of the psychophysical experiment to which we have already referred.

Not that Boring has set himself the problem of harmonizing his psychological point of view with that of others who hold different attitudes concerning the subject matter and method of psychology. On the contrary, the fact is that Boring's analysis of the psychophysical experiment is designed to substantiate the structuralist's position[3] as over against that of

[2]Is this possible? We believe not. Consequently the question here really is whether some philosophic or cultural attitudes are more conducive to the obtainment of correct results in psychology than others. Our suggestion would be that we might very profitably give up all cultural backgrounds which commit us to the procedure of transforming the fact of pressing a telegraph key, when an agreed-upon colored paper is shown, into a mentalistic fact before we can describe it. From this standpoint we look upon the structuralist's reiteration of the distinction between "Kundgabe" and "Beschreibung" as a "defense mechanism." We discuss this problem in more detail later.

[3]Frankly we anticipate that the reader may say concerning the writer's present paper, "Well, here is another view to accept instead of our own." Our reply would be that we are not at this point interested in advocating some type of psychology rather than another, but rather we wish to raise the question whether psychology does not suffer from the fact that psychologists are committed to some preinterpretation or other of their facts before they are studied. Also, we wish to make the more positive suggestion that all interpretations should be derived from an investigation of non-prejudiced data.

the functional-mentalist, and the behaviorist who is presumed to be anti-mentalist. And so whatever symptoms of agreement we may find in Boring's discussion are not due to his desire to make peace with other partisans; yet overtures for agreement we do find in Boring's work nevertheless, and for the simple reason that he offers us an extremely valuable description of the psychophysical experiment.

In order then to test our hypothesis, that a critical study of the psychological experiment may lead to agreement between psychologists, the writer who stands not committed to either the introspective-structural or so-called orthodox behavioristic views,[4] and much less accepting any combination of the two, plans to make a comparison of the descriptions of a psychophysical experiment when made from the introspective-structural point of view and from what the writer assumes to be an objective point of view, based upon no other presupposition than that we are investigating some definitely observable fact. We shall therefore proceed to our comparative analysis of the psychophysical experiment.

III. The Analysis of the Psychophysical Experiment. Describing the two-point limen experiment, Boring says we have (1) stimuli in the form of compass-points at different separations. Next we have (2) perceptual patterns which pass through a series from a single sharp point to two separated points. These patterns Boring calls mental processes, or process material for the datum (structural) psychologist. Between stimulus and process there is "assumed" to be (3) an excitatory process (neural action). Finally there is (4) verbal report. This series of processes as a matter of detail Boring thinks it advisable to look upon as a dependent one, each phase a resultant of the preceding.

Reserving until later the question of the shapes of the mental processes we must next look into what appears to be a series of factors connected with the four above. In the visual schema which our author uses (p. 446) and which we hope the reader will consult, he places between stimulus and excitation the term 'stimulation,' saying: "stimulus gives rise to excitation by way of stimulation." We take this to mean merely that the stimulus or object gives rise to an excitation process by means of a stimulating act. Again, between the term excitation

[4]We sometimes wonder whether there exists such a person as an orthodox behaviorist (namely, one who thinks of the psychological datum as merely the action of muscles, nerves and glands) or whether he is a controversial artifact. If not, then the agreement of psychologists is more plausible then otherwise, and especially if it should turn out that there aren't any mentalists, that is, those who are committed to the view that the data of psychology are purely mental states.

and the term perceptual pattern, Boring puts the term 'attention,' and writes: "excitation culminates in perceptual pattern, under the selective action of attention." Now here Boring's meaning is not so clear; but we take him to mean that excitation is neural action or process and becomes transformed into or parallels a mental action or process. Finally, between the terms 'perceptual pattern' and 'report' Boring puts the term 'criterion,' with the following statement: "the perceptual pattern issues in a judgment in accordance with criteria of judgments that have been established." This statement we interpret to mean that the observer tactually perceives the stimulus and consequently is enabled to judge or say whether there was one or two points applied to the skin.

To us at least there appears to be a slight ambiguity in Boring's description, concerning which we might comment, since it is obviously essential that every item in our description must be carefully scrutinized. There are two points here. In the first place, Boring does not sharply distinguish what the experimenter does from what the observer does. The stimulation action is obviously that of the experimenter. To be attentive or inattentive is the action of the observer at the behest of the experimenter, as is also the work of observing the criterion. In the second place, there seems to be an attempt (at least verbally) to distinguish between two series of factors, one (stimulus-excitation-perceptual pattern-report) which does not stress action, while the other series (stimulation-attention-criterion) does. In the first series stimulus is certainly a thing and won't trouble us. Excitation is a condition and as an isolated process need not necessarily be called an action; report or judgment is clearly an action. But what of the perceptual pattern? Here we should like to have a more definite statement since our comparison of descriptions will hinge on this point. Criterion in the second series, although a noun in form, is, to judge from the context, without doubt meant to be an action.

Such then is the structural psychologist's analysis of the psychophysical experiment. Observe how an objectivist (let us assume that label for our present purpose) analyses the same situation. Our series of events runs as follows: we begin with (1) the stimulus; the next member of our series we call (2) the attention act. Then we have (3) a perceptual response followed by another act which we may call (4) an ideational, reflective, or judgment act. Then finally we shall have a final or consummatory act which we agree to call (5) verbal report.

Before attempting a comparison of our analysis with that of Boring we must point out that we divide our behavior situation into stimulus and response, the former consisting of the application of the compass-points to the cutaneous region in

question; in other words, the exertion of the pressure at a particular point. It happens to be true, of course, that in our experiment the stimulus phase of the situation involves the action of the experimenter, but this action is part of the stimulus. We may consider it as the setting of the stimulus, while the instrument-points we think of as the media of stimulation. According to the conditions of the psychophysical experiment as Boring describes them we must indicate that there is an additional or adjunct stimulus which consists of the instructions of the experimenter to be careful and to attend to the task at hand. Even without this addition it must be noted that the experiment would always involve two stimuli, one the pressure and the second the request to make a report. Were it not negligible for our present purposes we should have to include the warning to be careful as a second auxiliary or third stimulus. As it is we will try to keep the description as simple as possible. But we might add, however, that under certain conditions the first auxiliary stimulus we mentioned might be thought of as setting instead of stimulus.

The second phase of the act we call the response, and possibly in every case but a reflex action it consists of a pattern, namely the series (1) attention, (2) perception, (3) judgment and (4) verbal report. As to the interdependence of the members of the series we should agree that under definite experimental conditions and with a trained subject they may be practically dependent one upon the other, but it is certainly true that this is not always the case. For, as the experimental data all show, numerous errors in perception and judgment occur. That a wrong perceptual act (ordinarily called an illusion or a false judgment) should occur is entirely due we believe to the fact that what should be an organized progressive series of responses and stimuli do not operate in their expected order. It is more common of course in practice that the judgment should not follow its precurrent perceptual act than that the perceptual act should become dissociated from the stimulus. Under ordinary conditions also the verbal report might be expected to depend upon the judgment; but it might not, and certainly all kinds of slips can displace the connection of judgment and stimulus.

As to the nature of the response each member of the series is a definite adjustment of the person; in principle then all members are alike, being actions of the individual, but they differ in function and form. For us the verbal report is an act precisely like those called judgment, perception and attention; each consists of perhaps a single reaction system. We are especially eager to make this fact clear, for it is precisely in these details that the agreement or disagreement of psychologists

upon fundamental problems is possible. Our response, then, in the psychophysical experiment we may say consists of a series of reaction systems.

IV. Points of Agreement and Possible Agreement in the two Analyses. Unfortunately upon several points a complete harmony of interpretation between the two descriptions will not be found; but it is certainly true that upon essential points a fairly complete agreement exists. Let us proceed then to a consideration of the likenesses and differences of the two descriptions.

(1) A primary agreement and a most important one is that both undertake an analysis of the experiment. In both cases the reacting individual is studied in the sense that we may expect to be informed how he is stimulated, how he must conduct himself with respect to his attention to the problem in general, to his own action and to the stimulus. The importance of this point lies precisely in the fact that only upon such a basis can the student of psychology approach his data with an unbiased mind as to what these data are. We shall presently see that to look upon the reactions of the person as the data to be analyzed and otherwise investigated may be the means of avoiding unnecessary and harmful presuppositions.

(2) Another common result of the two analyses is the conclusion that the observer in the psychophysical experiment performs a series of definite acts or events, what we choose to call a pattern of response. The only question that may arise here is whether Boring would agree with us that the perceptual factor is an action of the person, in principle like the verbal report, but different from it as the perceptual act differs from an attention action. To this problem we plan to return presently.

(3) That the series of acts or events in a psychophysical experiment response can be divided into different functional parts is another very important point of similarity between the two descriptions. The parts are the end factor (vocal report) and the middle factors (attention, perception, judgment), called 'middle' because they lie between the report end-factor and the stimulus end-factor.[5] In the first place, such a distinction allows us to look upon the different members of the reaction series as merely functionally distinct, but not as different kinds of facts; and in the second place, it provides a basis for the individual study of the different factors.

[5] Those who read this paper in conjunction with Boring's article (it was written with the assumption that it will be so read) will notice that I use terms which are not strictly mine or Boring's but rather derived from the two descriptions. I purpose, however, to guard carefully against ambiguities and especially against giving the impression that Boring accepts any interpretation of my own.

(4) We might turn now to a comparison of the specific members of the response series. To start with excitation. Here there is possibly a thoroughgoing disagreement between the two descriptions. For us excitation in the sense of neural action is not a separate factor but a constituent of every one of the four[6] reaction systems, to wit, attention, perception, judgment and report constituting the response. We do not conceive of any neural action alone preceding other kinds of action; but rather the neural action is a phase of the reaction system as a whole. If there is a deliberate disagreement between a structural view and an objective view here it is a very grave disagreement indeed, but we believe not inevitable. It is grave because, if the structuralist means to establish a permanent and actual difference between the neural fact and the mental fact (there is of course such a tradition), the structural position can never be reconciled with an objective point of view. That such a disagreement is not inevitable, however, we believe for several reasons. (1) There are psychologists who from an objectivist's standpoint are mentalists (Warren,[7] for example, who makes the two identical). (2) Boring himself must include a neural factor in several members of his series, attention and report for example. Whether he would care to admit this for perception we will discuss presently.

(5) Concerning the attention factor, this will not present us with any great difficulty, since attention being a fairly definite overt response the structural psychologist need not therefore differ so much from the objective psychologist as to what is involved in such action. We take Boring's inclusion of fatigue as a possible factor in the attention phase (p. 468) to support such a view. The same may be said when Boring undertakes to control attention by instructing the observer (p. 469). What else can "control by instruction" mean than that you ask the observer to do something of a somewhat different sort than to say "one" or to say "two?" Here we can certainly not avoid the fact that we are dealing with the mode of adaptation of our observer in the concrete psychophysical experiment. The writer is willing to make the mistake of assuming that no one will disagree that you can no more separate the mental from the rest of you when you obey the instructor's request to perform some particular act (speak) than when you are told to do something else(watch your action). In both cases you may attend to your actions besides performing them. Would it not be an unnecessary analytic sophistication to

[6]These types of reaction systems we should say, since the reaction systems themselves are of course abstracted from the actual event; there may be thousands of them actually operating.

[7]Human Psychology, 1919.

divide up the person into a mind or mental process on the one hand, and the interacting or paralleling bodily process on the other?

(6) Next we turn to the perceptual factor, which not only is the crucial point in our harmonizing enterprise, but is also cheerfully admitted by us to be a very difficult factor to handle. Both the structural and objective positions maintain or lose their value depending upon their ability to handle the problem of perception.

Because the perceptual response is fairly intangible, representing as it does in implicit form some previous overt response, and also because a perceptual reaction may represent a unique organization of vestiges of previously performed reactions, it takes on the character of being fairly uncontrollable and certainly difficult to observe either by the reacting person or someone else. Now it is in these ways that perceptual actions are different from the other more overt factors in the reaction pattern, and if the matter of observability is the criterion we may well agree that perception constitutes a relatively "central" fact. As we have already suggested possibly it is the presence in our psychological behavior equipment of such intangible reactions that lends such plausibility to a purely mentalistic conception. That is to say, we strongly feel that the idea that there exist in psychology different aspects of the same response to a stimulus, namely mental and behavioristic, is based upon the existence of the more refined perceptual and ideational response besides the grosser reactions, attention and so-called habit acts.

And now we may ask whether this admission of the difficulty in observing the perceptual factor because of its implicit character constitutes a basis for agreement of what is involved in the entire experiment. Or would a structuralist not agree in looking upon the perceptual factor as a complex reaction system, in principle precisely like the other factors? Our doubt here arises from Boring's statement that the perceptual pattern as he calls it has a shape or form (oval, elongated, double paddle, dumb-bell). While he calls this "process material," one gets the impression from reading his description that he does think in terms of material of some sort. The impression is deepened when our author writes that "excitation culminates in perceptual pattern." While we prefer (both in order to keep our minds open with respect to Boring's description as well as not to close the avenue to agreement) to hold that his statement means merely that the one fact is followed by another, the statement might be taken to imply that the perceptual factor is a kind of qualitative material or process transformed from neural material (current). At least the process that is involved

may be interpreted to be something other than the total action of the person, even if it be thought of as an action within him or within part of him.

If the structuralist can agree with the objective psychologist upon this proposition, namely that the perceptual factor is a reaction system, an adaptational act of the observer in the experiment, then the two points of view can be brought into substantial harmony. For our part, we cannot discover any insuperable obstacles to such an agreement, and in fact we believe that in the following considerations is contained convincing argument for such harmony.

In the first place, we are on common ground as long as the structuralists agree in rejecting the notion that a mental state or perceptual pattern is any kind of stuff or quality, in favor of the idea that they are dealing with processes or acts of the individual. Külpe calls this perceptual process sensitivity and sensible discrimination, implying that this element of psychological fact is an action of the person. When the perceptual factor is looked upon in this way the difference between the structuralist's view and that of the objective psychologist disappears. To us the entire perceptual problem seems clear when we are dealing with a complex action pattern which can be divided into separate attention, perceptual, final report and other reaction systems. But how about the case in which there is only a single reaction system? In that case attention and perception may be looked upon as integral phases of that reaction system. Perception is merely the cognitive or discriminative factor of a response system. From the standpoint of objective psychology the cognitive factor in a simple act, that is, one that involves a single reaction-system (reflex for example), is that peculiar coordination of response and stimulus which makes for a specific act. Cognition is the fact that different objects call out differential reactions in the organism.[8] In a complex act (many reaction systems) each one involves a cognitive factor of this sort, and besides one whole reaction system may be cognitive in its function.

In the second place, much encouragement for the possibility of reconciling the structuralist and objectivist do we find in the criticism made by the structural psychologist of the behaviorist and psychologist of capacity. In this criticism the structuralist implies that all he means to insist upon when he argues for a "central" factor in the psychophysical experiment is that he does not believe that in this experiment we merely undertake the physical measurement of bodily response as a function of the physical quantities of the stimulus. We may entirely agree with him that a bodily act (muscle and nerve

[8]Cf. a discussion of cognition in reflexes, this JOURNAL, 33, 25f.

action) is not a psychological datum and that the experiment must involve other factors. But the question is whether the introspectivist agrees with us that this central factor, which is an essential one in the total situation, is a complex response of the actual person and not abstract qualities of mind or consciousness.

(7) Turning to the final item in the reaction pattern, namely the verbal report, we need only remark here that the agreement is complete. Without any doubt whatever, the experimenter can look upon the report as a reaction unit comprising all of the factors that can be specified as integral phases of such a system.

V. The Problem of the Stimulus-Error. And now we are prepared to examine briefly the problem of the stimulus-error and how that problem affects the general inquiry into the possible agreement between psychologists. Heartily in accord are we with Boring in his insistence that not the best scientific results can be obtained by studying merely the response and the verbal report, for there is more than these involved in the situation. Let us frankly assert, as we have already intimated, that the psychophysical experiment cannot be understood and much less controlled without taking into consideration all of the factors (reaction systems) involved. In so far then as the problem of the stimulus-error means that we must not overlook the total series of response factors we must recognize the problem and avoid the error.

Let the reader observe, however, that to agree to study the middle factors in a reaction pattern does not at all supply any new element or support for a structural position, since as a matter of fact even the psychologist of capacity (Cattell) never denied the existence of those middle terms. For Boring tells us that the psychologists of capacity call these middle factors, attention, perception and judgment, inscrutable "middle terms." What they do deny is that these factors can be controlled and measured.

Boring, in his study of the psychophysical experiment, makes two assumptions which he believes materially support a structuralistic (datum) psychology. In the first place, he assumes that if we recognize the stimulus error and avoid it we thereby shall prevent ourselves from committing the psychophysical experiment to equivocal results. The second assumption he makes is that since the control of the response factors or the middle terms of the response conditions the validity of the experiment the structural hypothesis is thereby established. Now for our part we may readily and cheerfully admit that we get different results from the experiment depending upon whether the observer is paying attention to his response or not, as Friedländer's experiments on weight-lifting appear definitely to

indicate; but does this imply anything at all concerning the mentalistic character of the middle terms? Moreover, suppose that it be established that in some cases when the observer pays attention to his reactions he can offer more consistent and more predictable results,[9] does this fact alone establish that the middle terms are mentalistic?

Furthermore, as we have already written in a previous part of the paper we believe most emphatically that there exists a distinct difference between the perceptual member of the middle factors and both the preceding attention factor and the final verbal report. This difference we consider to be in large part that the perceptual reaction system is much less overt and consequently more capable of direct observation by the acting person than by anyone else. Again, we may repeat that we are convinced that the perceptual factor of the middle terms is much less easy to control than are the other middle factors; but in neither of these two cases can we agree that any support is found for a mentalistic point of view. Nothing in these two facts lends credence to the interpretation of our data in such a way as to separate the structural psychologist from the behaviorist or from the capacity psychologist.

On the other hand, we do believe that any suggestions concerning the possibility of controlling the middle terms in the psychophysical experiment must imply that they are something other than mental stuff of any sort, in fact reaction systems. Much confirmation do we derive for this belief from the fact that Boring himself does not appear to insist upon our ability to control the perceptual factor but mainly the attention phase, and the attention factor is by far the least equivocal reaction system for him; that is to say, it is apparently psychologically least mental in his estimation. This last statement we make however in full view of the fact that Boring apparently groups all the middle factors as mental in contrast with the verbal report.

What kind of psychological principle do we commit ourselves to, we might ask ourselves, when we recognize the problem of the stimulus-error and attempt to avoid it? Why simply this, that we accept the scientific fact that psychologists deal with different kinds of data from those of physicists, for example. In brief, the psychologist is interested in psychological facts,

[9] It is not at all scientifically established whether we get better results from attending to stimulus rather than to response in all cases. In our uncontrolled observation and especially in golf as well as other responses evidence seems rather conclusively to favor attention to stimulus.

to wit, the reactions called out by stimuli, that is the activities of the observer in the psychophysical experiment.[10]

At the risk of digressing from the main trend of our immediate exposition we must suggest that to consider the psychologist's data as responses to stimuli-objects, irrespective of how we regard the responses, is a gain toward a generally improved situation among psychologists and a step toward agreement. For we believe that the older mentalistic view that made the work of the psychologist consist of an analysis of the same objects from another angle from that which interested the physicist was certainly not correct. For example, Titchener believes that psychologists study the same objects that physicists study "with man left out;" but the former study those things "with man left in." The difference then between the two scientists it turns out amounts to this, in the view of the structural psychologist, that while the physicist studies the same objects as electromagnetic waves, vibratory motion or molecules, the psychologist studies them as looks and tones and feels, that is as sensations and feelings.[11] That the structuralist cannot be consistent in this attitude is plain when he adds to the sensations as psychological data feelings, thoughts, emotions, memories, imagination and volition,[12] for clearly those are the acts of persons. Moreover, the structuralist says that "the man left in" reduces to the nervous system, which makes his data closely connect up, if not identical, with reactions.

To the credit of the structuralist, it must of necessity be said that by making sensations and physical energy aspects of the same things he is attempting to avoid epistemological problems,

[10]We believe that at this juncture we get some light upon the argument of the anti-objectivists, to the effect that an objective psychologist has no right to assume any knowledge upon his part concerning his own responses. Because the objective psychologist assumes that he requires no idealistic epistemology in his work he is supposed to cut himself off from knowing anything about himself. The objectivist believes that when he observes his own reactions the knowing event is precisely as natural a phenomenon as when he responds to some other person's reaction. It so happens of course that when the psychologist experiments upon himself the responses that he observes are his own in two cases, to wit, (1) the reaction made when he offers the stimulus and (2) the reaction (his) to that stimulus; whereas when he experiments upon someone else he is interested only in the responses of the other person. The other person (observer) would, if the anti-objectivists were correct, be also cut off from knowing anything of his own behavior.

[11]Cf. Titchener, A Beginner's Psychology, 1915, 8ff.

[12]How the mentalist can hold such a view at all appears in our opinion to be accounted for by the fact that sensations are abstracted from the qualities of things and the other intellectual processes are made into derivations from sensations. Feelings of course remain outside this scheme and possibly this is the reason why psychologists have so strenuously attempted to make them into qualities or aspects of sensation.

but the difficulties he thereby creates are no less than those he overcomes. For he makes psychological data into peculiar abstractions as far as sensations are concerned, while the more complex reactions cannot be handled at all. And all this we believe can be rigorously avoided, as we have fully reiterated, by considering psychological data as responses to stimuli.

We come now to an extremely interesting phase of the stimulus-error discussion, namely the problem whether in fact the avoidance of the error means any slighter attention given to the stimulus even when one greatly increases his control over his own reactions to the stimuli in question. Interest attaches to this point because we insist that the control of the response involves at the same time a greater control over the stimulus. How can it be otherwise? For we believe most firmly that the psychological fact is the interaction of a response with a stimulus.

That the control of the response in the psychophysical experiment is at the same time a control of the stimulus, or at least involves prominently the stimulus, is evident when we consider that the errors in the experiment are not so much due to the neglect of the reaction as they are to the fact that the observer shifts his attention from his action to the stimulus and back again to his action. The only question here is whether a better control over the situation could be obtained by paying attention exclusively to the reaction, after paying attention to and perceiving the stimulus of course. In answer to this question we are not ready to say Yes, but we are thoroughly convinced that in any case what we are dealing with in psychology is a reaction to a specific stimulus and possibly we must conclude that it is always essential for both stimulus and response to be synchronously controlled.

Assuredly we find various evidences for the impossibility of minimizing the importance of the stimulus in the psychophysical experiment, and one of those is the fact that because the perceptual factor cannot be readily controlled, if at all, we must, when the problem of controlling responses is in question, place our emphasis upon the attention factor and the verbal report. Very significant is this fact since both of these factors are very definitely related to the stimuli factors of the experiment. Furthermore, unless we keep the stimulus under control as well as the reaction factors we cannot achieve any capacity to predict what responses will be made to stimuli. That this prediction must always continue to be very imperfect is due no doubt to an inability to control the perceptual process.

With the structuralist we believe we share this idea of the importance of the stimuli factors in psychophysical experiments, for Boring's insistence upon the control of the attention

factor may be definitely interpreted to stress this point. Also we find Boring's attitude favorable to our view in his belief that the control of the psychophysical experiment can be achieved by means of a criterion. For a criterion cannot be thought of as operating exclusively with a perceptual pattern but only in conjunction with the stimulus.

Let us notice that when we plan to control our responses to stimuli what we can do is to compare our own report, based upon our present judgment concerning what we have perceived with respect to the stimulus, with what we have judged a minute ago. But certainly it seems clear that we cannot control the kind of perceptual response we should make to any particular stimulus. Our control is bound to connect itself with more overt factors in the behavior segment than the perceptual reaction. Let us repeat, then, that what we do while paying attention to the response rather than to the stimulus consists of more carefully controlling our report with respect to the stimulus. Notice that we do not in any sense give up the stimulus, nor attach ourselves exclusively to the response, for the response doesn't mean anything without the stimulus. The condition might be entirely different, however, if we could assume that there is in us some quality which can be seen or otherwise known; in such a case we might consider that we could study it. But this assumption in our opinion could only rest upon an interpretation added to our observation in order to support a structuralistic position.

And now we hasten to add that nothing that we have written implies our rejection of the hypothesis that a verbal report follows a judgment response and that the judgment is based upon a perceptual response. What we do insist upon is that each factor constitutes a reaction system, a positive response of the person, and that all the response factors together make up a single reaction pattern to the specific stimulus in question with its experimental setting. To attempt to control the psychophysical experiment by attending to both stimulus and response results, in our opinion, in a closer connection between the perceptual factor and the stimulus.

VI. *The Data of Psychology.* We believe that the comparative analysis that we have made of the descriptions of the psychophysical experiment forces us to recognize that the data of psychology cannot be thought of as being anything else but actual responses of a person to specific stimuli. Any other way of describing the situation cannot be said to be an analysis of the facts in the case. Surely the employment of such terms as mental, physical or bodily, when intended as descriptive names, constitutes interpretations based upon some kind of assumption or set of assumptions. These assumptions we may

further say are not based upon observable facts whether found in this experimental situation or elsewhere. Rather, they are derived from some prepsychological or perhaps even prescientific domain of human thought.

Our analysis of the psychophysical experiment, which be it remembered we take to be in principle typical of psychological phenomena, has yielded, in our opinion, other results which locate the actual facts in the case and provide a basis for the agreement of psychologists. One of these results is that we may consider the psychological reaction to be made up of comparatively simpler unit reactions or that a large behavior or response pattern can be analyzed into smaller unit reactions.

Another fact concerning the data of psychology which we may accept as an actual product of our analysis is that some of the unit reactions in the psychophysical experiment are primarily overt responses while some are mainly implicit. In the latter instance the facts are in part not entirely observable and consequently involve some assumption. But here again the interpretation is we believe absolutely derived from the observed data and is essential, at the same time that it is supplementary.

In support of our contention that the data of psychology are in fact complex responses of organisms to things and not mental states or aspects of things, we may adduce the fact that in spite of psychologists' contrary theories their practice has always been based upon a reaction hypothesis. Take any book written by a professed structuralist and what do we find? There is not a single psychological activity which is described other than as a response to a stimulating condition. This is certainly true, is it not, of the simpler facts treated under the heading of reaction time? And when we come to sensations are not these always presumed to be abstractions from the adjustments of organisms, abstractions consisting of the differential factors removed from the rest of the adjustment action? This we assume is what is meant in part by psychologists who consider sensations as merely abstractions from perceptual acts, or when they consider sensations as correlates of receptor and neural functions. Moreover, unless the structuralist considers sensations to be phases of reactions would he not be inevitably and hopelessly committed to stimuli-errors? We come, then, to the conclusion that the interpretation of psychological data as anything other than responses to stimuli is the result of an influence extraneous to psychological analyses and in fact is the outcome of peculiar philosophic preconceptions. It is hardly too much to say that the entire psychophysical problem with the bitter battle it entailed concerning the quantification of mind was due to a preinterpretation of the

facts prior to a fair study of them. What makes the situation more difficult than it otherwise would be is that philosophic preconceptions are not knowingly accepted but merely taken over with the rest of the cultural traditions of the time. This condition prevails to a certain extent today and was the dominant circumstance in the earlier days of scientific psychology.[13]

Does not the very term psychophysics stand as a symbol that the work and method of scientific psychology was prejudiced at the outset? And was it not this prejudice that was responsible for the unending strife concerning the stimulus-error and whether what was assumed to be the psychological datum could be measured?

From the standpoint of the reaction hypothesis it is almost tragic how the various students of the psychophysical experiment have misinterpreted the more implicit elements of the reaction system of the psychophysical reaction and thus precipitated the controversies as to what was measured or whether what they wanted to measure could be measured. And so as to the identity of the datum; it was either made into something that manifested itself in action (verbal report, Fechner) or it was presumed to be a parallel or manifestation of nervous action (G. E. Muller), or again the datum was transformed into a judgment (is this not true of most psychophysicists?). In every case we believe the actual thing studied, namely, some part or the whole of the reaction to a stimulus, was interpreted to be something other than it actually is instead of being described as an existing fact. And most unfortunate it is that scientific or experimental psychology still carries with it the evil effects of not having been begun as a critical investigation of concrete facts.

VII. How Metaphysics Influenced Psychological Interpretations. Most clearly can we see the modus operandi of the philosophical background in experimental psychology when we look back upon the early development of the psychophysical experiment and scientific psychology in general. Now what do we find? At the beginning of the development of scientific psychology, when "psychophysics" as a technical scientific term was developed, the primary philosophical interests of Europe were still concerned very definitely with the epistemological problem how the process of knowing can mediate

[13]We wish to eschew absolutely the implication that we believe that the mistakes in early psychological work, if such we admit, could have been prevented, or that some individual or group of individuals is responsible for whatever difficulties one admits to be present in the early experimental work. On the contrary, we believe that the conditions attending Fechner's work made that work historically inevitable. But our whole point is, must we forever burden psychological science with errors that crept into it in its infancy?

between a known material object and its psychic correlate, and the question how the knowing process can reproduce in consciousness what is apparently a totally different form of thing. This interest, of course, arises from the theologico-metaphysical background which implies a diremption between spirit and matter, mind and body, and the mental and the physical. It was in the interest of this metaphysical problem and not in order to investigate a definite psychological problem of response or adaptation to a stimulus that Fechner undertook his work on the psychophysical problem.

In point of fact, Fechner entered upon his activities with regard to the psychophysical problem in an endeavor to establish definitely the identity of mind and matter (by the employment of the Herbartian suggestions of mathematical expression and his threshold mechanics) and not to investigate any psychological problem.[14] As a result his work was the target for a host of criticisms which could only be effective because Fechner sought to establish his religious or metaphysical theory by the utilization of physiological data. In other words, we might say that Fechner through his metaphysical interest prevented a more definite and more scientific interpretation of the sensory psychological data available and a more substantial research for more data.

But if the scientists who came after Fechner were more interested in the psychological facts than in the establishment of a metaphysical theory, their work still suffered from much the same philosophical bias. Though psychologists undertook to study assiduously the facts of association, memory and perception they did so with the assumption that they were bringing together the psychical and the physical. Experimental psychology, especially, consisted of bringing together physiological data with facts of knowing, feeling and willing which are in some sense different from the former. What was the result? In the case of Wundt's psychology (to mention one example which has had far-reaching effect upon current psychological thinking) the metaphysical influence was responsible for the misguiding of a voluntaristic point of view, which originally promised to study psychological phenomena as derived from a biological matrix, that is, from observable facts, into a hopeless intellectualistic abstractionism which Wundt himself deplored in Herbart and his predecessors.[15]

To our way of thinking the whole gamut of evil results brought on by the metaphysical influence upon psychology

[14]Says Wundt, in his address on Fechner (Gustav Theodor Fechner, 1901, 84): "Das ganze Interesse Fechners gehört eben nicht der Psychologie als solcher an, sondern diese ist für ihn nur ein Bestandtheil der Natur- und Religionsphilosophie," etc.

[15]Cf. Wundt-Titchener, Principles of Physiological Psychology, 25ff.

is that psychologists can still think of themselves as dealing with epiphenomena.[16] And just here can we find one of the most serious evils of attempting to interpret psychological facts in metaphysical terms. Since there can be no possible relationship between the observed facts and the interpretations, the facts are not interpreted at all and thus are blindly handled or else they are misinterpreted and in consequence are far removed from any progressive scientific development.

Is it too much to believe that if psychologists had started out from the beginning of their science with the study of the psychophysical experiment or the reaction time experiment as an existing fact they never would have made the division between mentalism and behaviorism or between mind and body? In our opinion this particular division is a cultural heritage from a definitely pre-scientific period of intellectual development; possibly it is merely a theological conception. The writer would suggest that in the present stage of scientific attainment we need have no such dualistic or monistic conceptions as the mental-physical dichotomy seems to indicate. Emphatically we may suggest again that to reject all philosophical preconceptions means that not only shall we not lose thereby the distinction between psychological and non-psychological facts but on the contrary we shall achieve a much more sensible form of distinction.

The untoward influence of metaphysics upon psychology has really been twofold, the first of which, to wit that the datum of psychology comes to be considered in theory at least not as a reaction to a stimulus, we have already studied. Now the second deplorable effect of spiritualistic philosophy upon psychology has been such that, even where the psychological fact is either in theory or practice considered as a reaction to a stimulus, the reaction is not looked upon as a concrete reaction or adaptation of the natural organism, but it must be interpreted as a mental or spiritual thing or process (epiphenomenon). We may turn now to an examination of the second of these misinterpretations of psychological data, which compels us to interpret them as anything but complex organismic responses to stimuli.

And first we might consider the peculiarly intimate character of human psychological phenomena. Every psychological reaction is in a definite sense a very particular kind of event; it is the action of a human individual, a person with a unique value of his own. This fact is recognized in one of the cardinal attitudes in psychology, namely, that the fact of individual differences in the cornerstone of the science of psychology.

[16]Cf. Washburn, Introspection as an Objective Method, *Psych. Review*, 29, 1922, 89ff.

Moreover, we may well grant that much as we may know concerning the response of another person to a stimulus, that action is in a unique sense bound up with that person and his past experience. But yet this fact does not in any sense support a mentalistic interpretation; in fact we believe it even argues for an objectivistic interpretation.

Does the intimacy of an act make it anything other than a natural phenomenon? To answer this question we must first ask what we mean by intimacy. Do we imply anything more than that in a given time and place some specified event has happened to some particular individual? Now unless we especially desire to give our data a particular interpretative coloring we cannot overlook the fact which the events themselves present us with, namely, that if the event happened to a stone or any other physical object the event would be no less a unique occurrence of a natural fact. As a detail of scientific methodology the more accurately one can determine the spatial or temporal setting of an event the more elements one has to add to an objective study of the phenomenon no matter what particular sort it may happen to be.

A second characteristic of psychological facts which probably lends plausibility to the mentalistic interpretation of psychological data is their fleeting character. Because psychological facts are intermittent processes of brief duration and not subject to reappearance they are considered as different from other natural events; they are considered as psychic and not physical. That psychological facts are fleeting actions, that is again to say unique events, cannot be denied, but that this fact involves any sort of different situation than any other natural fact is preposterous to believe. Is it possible to revive any event, let us say the particular chemical reaction which I have just performed? It is true, of course, that some kinds of facts are so comparatively simple that we may, for practical purposes, say the securing of measurements, consider them as the same, although they are different events. But is this circumstance any different in the field of psychology from what it is in that of the other sciences? That even the simple physical facts are never the same and cannot be exactly repeated we learn from the pervasive and necessitous employment of the statistical method in the physical sciences.

Again the striking fact that some of the data of the physical sciences involve reversible processes may have strengthened the philosophical influence upon psychology. Some chemical reactions for instance are almost reversible, that is to say, such similarity is found in the things transformed or retransformed that the same event may be said to occur. An examination of this case impresses us with the fact that even if the measurements made give us the same results without averaging, which

of course is hardly the case, we could not consider these as repetitions of the same event without abstracting from the temporal variable. Furthermore, is there so much difference after all between the reversible chemical action and some forms of psychological phenomena? Certainly any language or learning situation shows a reversible reaction and a repetitive one.

Possibly by far the most potent source of mentalistic interpretation can be traced to the facts of self-observation or introspection. By this we mean that in human reactions there are certain ones which cannot be observed by anyone but the acting individual. Let us take a concrete case. From my window I see some person come out of the next building, walk about a hundred paces to the right, then turn around and walk back into the building. Soon the individual reappears with several volumes under his arm. We may be fairly certain that what we have here is a memory reaction. Apparently the person forgot to take the books he planned to take when he first left the building. Of course it is barely possible that this is not a memory act, since it might be true that the person did not go back for the books but for some other purpose. Seeing them, however, he decided to take them home. But at any rate, and this is the important point, the fact that this action may from the standpoint of the observer fall under two different classes, and for the reason that the observer cannot tell what the stimulus is, indicates the dependence of the description upon the reacting person.

Just what is the adequate stimulus in many cases the onlooker cannot know, even when he is sure what sort of reaction situation is being worked out. Suppose we put our illustrative individual under controlled conditions so that we may know he is performing a memorial reaction. Now in the case we have chosen, what is the stimulus that evokes the memorial reaction? It is possible that the person is performing the memorial response because he was stimulated by some books carried by another person. These books may serve as a substitute stimulus for some books he wishes to carry home, which books constitute the adjustment stimulus. On the other hand, it may be that this individual was stimulated by looking at a tree. The tree may operate as a substitute stimulus because one of the forgotten books contains some information on trees which the individual is interested in. Thoroughly to understand the behavior situation it seems necessary to know what the stimulus is in this case, and yet we cannot normally have that information. Again, in some cases the stimulus cannot be known to any one but the reacting person. This necessity of knowing what is the stimulus, let it be observed, exists only for us who wish to describe and understand the reaction and not in order that it be possible for the reaction to occur. It is

of course probable that while the memory reaction cannot occur without the substitute stimulus, the reacting person himself may not know what it is; it merely occurs to him, so far as he can tell, that he must return for the books.

What is true of the onlooker's knowledge of the stimulus phase of our illustrative behavior is always true perhaps concerning some phase of the reaction. But we question whether it is anything but a metaphysical presupposition which on this account takes the memorial act out of the domain of natural phenomena. For how many factors in the events which exclusively involve purely chemical and physical behavior are we always ignorant of?

And is not precisely this the function of the scientist, namely, to discover as many of these conditioning factors as he can in any kind of event, in order to give definiteness and authenticity to his observations and subsequent interpretations? The mere fact that we do not know what are all the conditions in this person's action certainly doesn't transform it into anything other than an objective event. This fact is obvious if we consider that the same situation prevails with respect to man's digestive conditions. Is it any less a purely objective fact that man digests food and a particular kind of food, if only the digesting person knows how and when this particular event is happening?[17]

But we have yet to face the problem of the cognitional uniqueness of human reactions. That is to say, a very distinct difference may be observed between what we call physical or natural events and what we term psychological facts in that the latter involve a knowing element. Not only does the person react to a stimulus, but he may know he is doing so.

In the first place, the question arises whether the fact of knowing is a mark of difference between psychology and other kinds of facts, because, as we have just indicated, many features of the individual's response may be unknown to himself, and this refers not merely to the stimulus side of the factors but to the response side also. Here we need only refer to the occurrence of reflexes and a host of responses popularly called subconscious and unconscious. As a matter of fact, then, the knowing of the action by the person who performs it is not an essential factor in the event; and therefore a psychological

[17]Although Washburn (*loc. cit.*) assumes that her discussion of introspection supports an epiphenomenalism, the objectivist cannot be aught but encouraged by it, since even a professed structuralist means nothing more by introspection than objective description. Washburn's epiphenomenalism turns out to be, then, a matter of arbitrary preference, based perhaps as she suggests upon early training. We must, however, look upon her paper as a step toward the meeting of extremes.

event from this standpoint cannot be interpreted as a mental thing different from what is called a physical thing when that term is synonomous with a natural event.

But then we do have, of course, responses in which knowing is an essential factor, and our inquiry may be directed toward the question whether knowing itself as a fact is mentalistic in the sense of a non-naturalistic phenomenon. Before asserting our disbelief in this proposition let us ask whether we can get some light on this problem by inquiring why knowing is presumed to be mentalistic. This inquiry is stimulated by the fact that there appears an obvious difference between the act of picking up an object, say, and the act of intending or desiring to pick it up. Here again we must ask why the apparently "internal" implicit act is different in principle from any other kind of action. We are firmly convinced that the knowing or internal act is distinctly a response to a stimulus just as the picking up response, and moreover, a response to the same stimulus. Now the difference between them is that one we look upon as precurrent while we consider the other to be a final response.

The hiddenness of an act should be no inducement to think of it as a mental thing because certainly nothing could be more hidden from observation than the act of digestion. Obviously, we can know both events, at least in some cases, by particular means of experimentation. There yet remains the fact that the knowing is the action of the same individual who performs the final act. This point does not add anything more novel to the situation than the points we have already discussed. Our point is that all of the acts of the person, the knowing and the final overt (motor) responses, are purely definite natural events that happen to that particular individual.

VIII. How the Data of Psychology compare with the Data of the Physical Sciences. Remains yet the question, if the data of psychology are considered to be natural facts in principle like the data of the other sciences, wherein then lies their specific differentiation from the latter? So far as scientific investigations are concerned we may say that all scientists are interested in particular kinds of natural objects or things and the way they behave. For our present purposes[18] we might classify scientific facts into three general types, namely, physi-

[18]Our present purposes are such that we neglect the fundamental principle that there are as many kinds of scientific facts as there are scientific interests and information.

cal,[19] biological and psychological. The physicist studies actions of physical things (leaves, stones, dynamos, light, etc.) and the actions of these things he finds he can sum up under various laws, the Newtonian laws in mechanics for example.

The biologist studies actions of things (organisms) which have different qualities from the objects which occupy the attention of the physicist. Biological things behave on the basis of stored up energy (action), facts which may be symbolized by the terms metabolism, irritability, etc.

Psychologists likewise study a type of natural thing, namely some person or animal.[20] What is of interest is that the psychologist studies behavior which differs in various definite ways from the behavior classed under the two preceding types.

For practical purposes we may attempt to sum up the essential psychological character of behavior under the six following headings, each involving a different relation or interaction of a stimulus and response: (1) variability of reaction, (2) differentiation of reaction, (3) modifiability of reaction, (4) integration of responses, (5) delay of reaction, (6) inhibition of reaction.

(1) *Variability of Reaction.* When a psychological organism comes into the range of stimulation by an object, the relation or interaction between the two continues to a definite conclusion, frequently with a great variety of movements upon the part of the organism until one of several things happens. That is to say, until the organism (*a*) changes its relation with respect to the stimulus; (*b*) produces some effect upon it; or (*c*) the stimulus disappears or becomes invisible; or (*d*) the animal is fatigued or exhausted. The number and intenisty of these variations are determined of course by the specific details of the situation. (2) *Differentiation of reactions.* At the basis of the variabilty of behavior as well as of some other marked action characteristics is the process of differential reaction. In a general sense, we may say that different specific objects and persons or qualities of them produce different effects upon the organism, which in turn exhibits a differentiative sensitivity toward those objects and qualities. (3) *Modifiability of reactions.* In general the characteristic of modifiability of

[19]Here we must make sure that our language does not betray us into a nest of difficulties. The term 'physical' is here used in a technical sense as the domain of the physicist. When the term 'physical' is taken to mean natural as over against the metaphysical world of epiphenomena psychology, of course, constitutes a branch of the physical sciences. If the writer may offer his personal opinion the world of epiphenomena is nothing but a series of verbal constructions.

[20]From a psychological standpoint the thing studied may be considered as primarily a series of reaction systems to specific stimuli.

response permits the organism to profit by its previous contacts with those objects and in consequence allows the individual to develop a growing power over his surroundings. Thus we might say that the modifiability of behavior is a genuinely acquisitive process; the child, for example, who modifies his original hand-extension reaction to the candle flame acquires a withdrawal response or in other words learns to withdraw or not to extend his hand into the flame. We may think then of the characteristic of modifiability as a discrimination of stimuli, discrimination conditioned by previous reactions to those stimuli and also by present variation in the environmental settings of those stimuli objects. (4) *Integration of Responses.* Not only does an organism perform differential reactions to objects and modify those reactions but it also summates these responses. The psychological organism develops complex response systems which can be definitely observed to be combinations of simple reactions. As an example of this integration we might consider the development of speech. At first the infant learns to make single verbal responses to objects. Later the single word responses become integrated into sentences or comprehensive language reactions. (5) *Delay of Reactions.* This is brought about in two ways: first, the stimulus excites a reaction system or series of reaction systems which function incipiently until such a time as an overt response can be performed. Let us observe at once that the incipient acts may be overt responses in the sense that some direct action is performed upon the stimulus-object, but the final overt act is the significant one. Thus, in the case of a delayed reaction of an elementary sort, some overt act in the form of a posture, or bodily set or orientation, will serve to fill in the gap between the stimulation and the final response. Or secondly, some object other than the original stimulus but which substitutes for it can at some later time excite the person to respond to the original object. In the first case, the various positive attitudes of the organism may serve as preliminary reactions to the final act; they are then called precurrent responses. In the second case, there is an actual discontinuity in the behavior of the organism. Both of these forms of delayed reactions constitute phases of memory behavior, the latter of which is of course the more complex and the more serviceable to the organism which is able to react in that way. (6) *Inhibition of Reaction.* Another characteristic of psychological action is the inhibitory process. Let it be carefully noted here that by inhibition we do not mean non-action but merely preferential reaction. That is, there is a differential and preferential operation of the various reaction systems which the organism has acquired. For various reasons some stimuli may become prepotent over others, as in the case

of a more favorable setting which they may have, or because they have just operated upon the organism which is now especially sensitive to them.

So far as human psychology is concerned we may say without hesitation that the datum is a response of a person. Now if we agree to dispense with any kind of metaphysical implication we mean by person nothing other than what is implied in our everyday speech. We do not go behind the person to a metaphysical substance called the soul or mind, nor do we reduce him to the abstraction of a body. Rather we consider him as a complete psychological machine operating in contact with stimuli exactly in the same way as the physicist considers a dynamo.

Recent investigations made with human individuals both normal and abnormal have resulted in a conception of personality which shows fair promise of developing into a point of view capable of doing justice to all of the complex and intricate facts of human adjustment. The analysis of this individual in action yields us much information concerning both the simpler and more complex unit reactions, which under the name of reaction systems are abstracted from the actual behavior of the person in his surroundings. In our estimation the psychological conception of personality refers to the particular group and sum of reaction systems that can be described as adaptable or maintenance reactions in their simpler forms and as economic, aesthetic, literary, cultural, intellectual, moral, etc., when the reactions are very complex.

The merit of these abstractions lies merely in the fact that they are directly derived from the observable behavior of the person and they can be further reduced for more minute investigations to the simpler muscular, neural, discrimination and other functions of which they may be said to be composed.

IX. How can Psychology be Emancipated from Metaphysics? That psychology needs to be relieved from its entangling alliance with spiritualistic philosophy is manifest from the unfortunate results which accrue from such a connection. As we have been attempting to point out, psychology's contact with metaphysics may be summed up in the proposition that the data of the science have been taken out of their actual concrete setting and transformed into various sorts of abstractions. This amounts to nothing less in practice than that the data of psychology are prejudiced before they are studied. The method of doing this has been as follows. Starting with the assumption that our stimuli consist of radiation of various sorts, psychologists have had to suppose that the colors, tones and other qualities of natural things could be created or transformed in the mind, from the light rays, etc., by means of a

nervous system. Consequently the psychological reaction had to be reduced to the process of "being qualities" (sensations). This attitude was formulated in the statement that the "mind" consists of these processes (sensations), etc., as over against the body and the stimuli. That the nervous system could be spared from reduction to radiations and mental qualities was only made possible by the psychologist becoming contemptuous at this point of the self-same metaphysics which brought on the problem. In the meantime, of course, psychology is not occupied with its own essential problem, namely the adaptations of persons to things.

And yet what appears to be the psychologists's deplorable occupation with the abstractions which we have been discussing is not without the encouraging feature that the whole procedure represents an attempt to place psychology upon a critical scientific foundation. In fact, this whole tradition of abstract psychology has been developed especially by Wundt and his followers in an admirable scientific spirit. And so psychologists seized upon what at the time of experimental psychology's birth were definitely established scientific facts, namely light rays, etc. The unfortunate feature of the situation, however, was that the metaphysical influence urged psychologists to correlate the abstractions of physics, taken to be the material world, with comparable mental abstractions, namely sensations, which were assumed to be the mental correlates. Because this was all done in order to make psychology into a science, however, the encouraging fact remained, namely, that the way was never closed to a modification of point of view which we believe is now making rapid headway.

Once more let us emphasize this point, that the abstractions we have mentioned, although necessary and valid for the physicist, cut the psychologist off from the study of the concrete reactions of human and infrahuman organisms to air, water, food, mates, art, science, etc. Are not light rays, air waves etc., in general very remote from actual psychological facts? For we assume that it is light and not light rays that have to do with stimulation, and furthermore light itself does not ordinarily constitute stimuli but is really the medium of contact or stimulation. Of a surety light and light rays may be stimuli-objects for the physicist when he studies them; but this is a special sort of psychological fact. Most certain it is that no science can go far without abstractions, without cutting its data up into bits which may little resemble the original wholes, but our entire insistence is that so far as psychology goes the gross

data should be the facts of the psychological domain itself.[21] For otherwise we may borrow abstractions in quantities from physics, from chemistry, and from other sciences, but they will serve not one whit to illumine our problems or to aid in solving them; since the facts of psychology, as we have seen, are clearly distinct from those of physics and the other sciences.[22]

Let the reader be warned that we do not base our argument at any point upon the necessity for achieving practical results in psychology. We are not interested for the moment in applying our scientific principles, although it is undoubtedly true that the sounder our principles are the better results can be achieved when we do apply them. Every psychologist must be struck with the success of certain features of the Freudian psychopathologists, those features we mean which have undoubtedly thrown much light upon psychological phenomena. No, our suggestion is made entirely for purposes of understanding psychological facts; for purposes of analyzing our facts we ought to approach nearer the actual occurrences. Let it not be said that the technical psychologist understands much less about human beings and their behavior than novelists or students of hysteria. In this connection we might suggest how much better off psychologists would be if they paid more attention to the concrete conditions of memory and thinking (as some psychologists are beginning to do) than to anything that has to do with the mental and the physical. For we may plainly say that there is nothing in nature which is mental or which is physical in any other sense that that natural phenomena can be divided into psychological and physical phenomena. Scientific psychology, in common with all the other sciences, selects its facts from the common source of natural phenomena and these facts comprise physical reactions, biological and geological facts, psychological responses, etc. The only use we can make of metaphysical abstractions is that by their employment we may dissolve facts into metaphysical concepts which we can in turn use to bolster up the metaphysical background of our science. Should such a background be tolerated?

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[21]This point refers to all abstractions: we are no better off when we take our abstractions from the biological domain than from that of physics. The so-called orthodox behaviorist takes his abstractions from the biological world and for this reason appears to keep closer to actual facts than the introspectionist who denies that sensations (color and other qualities) are abstracted from objects. For muscle action, nerve action, etc., are clearly abstracted from observable data. But even so the behavioristic abstractions are not of great value in psychological investigations.

[22]It is indeed gratifying to read in the newer textbooks overt statements with respect to the distinctness of the scientific field of psychology. Cf. Woodworth's treatment of color reactions in his *Psychology*, 1921, 210 ff.

Most firmly do we believe that all scientists theoretically appreciate the disservice brought to science by metaphysical presuppositions, and also that it is common knowledge that no scientific enterprise can be carried on without some fundamental assumption. We are in duty bound, then, to draw up a closer bill of particulars in our protest against the metaphysical wrongs committed in psychology. Let us specify, therefore, that we mean to rid psychology of metaphysical or transcendent philosophies and complete the emancipation of our science by retaining only or developing a logic or methodology of science. What would be the difference then between these two? Nothing less than this, that instead of entertaining presuppositions concerning the nature or identity of things before anything is studied, the psychologists will develop assumptions concerning the operation and meaning of psychological facts actually observed. Only from such assumptions and hypotheses may we hope to develop methods of discovering new data and newer methods of studying them. Incalculable would have been and could still be the application of such methodological principles in the study of instincts, emotions and intelligence, as the history of these problems amply testifies.

But here again we are entirely willing to agree that our suggestion contains nothing new. In fact Wundt makes a great deal of metaphysical presuppositions and methodological processes, but the question is whether his suggestion is in fact acted upon. The criterion lies here. When I perform a psychological act is my action divisible into a physiological part and a psychological part? We believe not, when we approach the fact without prejudice. That psychologists can agree upon this is eloquently testified to by the following quotation from a writer whose psychological position stands at the opposite pole from that of the present writer. Says Wundt: "the life of an organism is really one; complex it is true, but still unitary."[23] In spite of such a statement the metaphysical influence is responsible for the division of an action into mental and physical parts. We have then the result that while Wundt looks upon the mental processes (mind) and the physical processes (neural action, etc.) as conceptions, that is scientific interpretations of the reaction, he soon makes psychology into the science of mental states which are clearly different sorts of facts from the neural, although he still insists they are uniformly "connected" with those neural facts. Wundt's scientific path is here made smoother by the fact that according to his philosophical position both the neural and the psychic facts are ultimately reducible to homogeneous spiritistic stuff. Furthermore, now that the reaction is divided into physical and mental the same division

[23]Physiological Psychology, I.

is made to apply to stimuli objects, and a (perceived) tree, for example, is made to consist of a natural science part and conscious content part. The total results of this situation are in our opinion inimical to the interests of scientific psychology.

X. *Conclusion.* In concluding our paper we might briefly summarize some of the salient points that we have attempted to investigate.

In general, we might conclude that it is most essential for the purpose of psychology that the various workers should agree upon fundamental propositions. For in psychology, as is not true in other sciences, the disagreements go back to the primary data of the science, the things observed, and do not involve merely interpretations of things studied. As a consequence, psychologists not agreeing upon their fundamental data are much occupied with preinterpretations which for the most part unwittingly play havoc with both the facts and the science.

That psychologists might agree upon their data and primary methods we have concluded from a comparative study of a subjective and objective analysis of the psychophysical experiment. In each case the facts studied are first approached as definite responses to carefully controlled stimuli. Possibly in any careful study much the same agreement could be accomplished with respect to interpretations, but this condition, while very desirable in the interest of the science as a whole, is not of equal moment with the necessity of agreement with respect to fundamental data.

On inquiring as to the conditions which make possible such violent disagreements as we find in the domain of psychology, we may trace the difficulty back to a traditional acceptance of a faulty philosophical background. To divide an organism's action into mental and physical and to make the data of psychology into anything but responses to stimuli is not a scientific enterprise but a metaphysical one. The way to clear psychology of such metaphysical obstacles is to eliminate all transformative presuppositions and to make the hypotheses and assumptions refer to methods of investigation and interpretation rather than to variant existences.

MOVEMENTS OF PURSUIT AND AVOIDANCE AS EXPRESSIONS OF SIMPLE FEELING

By Paul Thomas Young, University of Illinois

In a previous experimental study[1] (conducted at the University of Minnesota) it was concluded that unpleasantness is associated with a widespread bodily response while pleasantness is lacking, for the most part, in organic and kinaesthetic processes. "With U there are withdrawing movements, frowning, straining, reflexes of expulsion, etc., while P is characterized by mere acceptance of the situation and the passive yielding to it.....P is felt when one relaxes, or simply 'does nothing.'" The data furnished abundant evidence for the correlation between U and movements of avoidance; but they cast a shadow of doubt upon the generally accepted correlation between P and movements of pursuit.

In a recent study[2] Corwin has advanced the criticism that our failure to find seeking movements as involuntary expressions of pleasantness is the result of experimental conditions and methods. Referring to our original study she raises the objection that the subjects "were seated throughout.....(the) experiment in a comfortable chair, they were already relaxed, and therefore it was easier for them to be 'passive and receptive' with P than with U stimuli. Their bodily comfort was a source of constant P stimulation." But a more important criticism, she continues, lies in the inadequacy of experimental conditions for producing P responses. "If an *O* has a P stimulus placed under his nostrils, there is no incentive for him to make seeking movements, unless *E* begins to withdraw the stimulus. Indeed, when we repeated the Olfactory Experiment under Young's conditions and instructions, we found no definite seeking movements or tendencies to move."

Corwin, therefore, modified experimental conditions. Instead of making her subjects comfortable in a Morris chair she used one of the ordinary type in which their bodily position would be a matter of indifference. She arranged "a situation in which the *O* must seek if he desires to retain a P." There were three experiments. (1) Vials of P and U smell substances

[1]Young, P. T., Pleasantness and Unpleasantness in Relation to Organic Response, this Journal, 1921, 32, 38f.

[2]Corwin, G. H., The Involuntary Response to Pleasantness, this Journal, 1921, 32, 563f.

were placed upon the end of a moving rod arranged in such a manner that the stimulus would recede from the subject's nostrils at the rate of 1.7 cm. per sec. In order to get graphic records a band, attached to the subject's head, was connected to an ergograph the marker of which wrote upon a smoked kymograph sheet. (2) In a cutaneous experiment P and U stimuli were drawn slowly across the subject's forehead while *E* observed whether or not there were following or avoiding movements. (3) Agreeable and disagreeable music, and other auditory stimuli, were led to the subject's ear through a rubber tube from another room. One *E* controlled the source of sound while another "moved the free end of the tube gradually away from *O*'s ear."

From the introspective reports and the graphic records Corwin concludes "that definitely seeking or maintaining reactions to P stimulations are found in 84.3% of the total number of P cases."

For two reasons it seemed advisable to repeat Corwin's experiment under slightly different conditions. In the first place, her moving stimuli suggested that the pursuit movements which were found may not be genuine expressions of pleasant feeling, and that another interpretation may be possible and perhaps preferable. Corwin herself notes that pursuit sometimes occurs "for another reason than P." In the second place, she has not treated the data comparatively by showing the number of times pursuit was found with U and avoidance with P; and the study is, consequently, one-sided and incomplete. Accordingly we have taken for an experimental problem: movements of pursuit and avoidance in relation to P and U. By 'pursuit' is meant any movement toward the stimulus-object; by 'avoidance', any movement in the contrary direction. Is pursuit, as a matter of fact, correlated with pleasant feeling and avoidance with unpleasant feeling?

I. Experimental

The following experiment was conducted in the Psychological Laboratory, University of Illinois, in the fall of 1921.

The subject[3] was seated in a white enamel, straight-back iron chair. One end of a tape was attached to *S*'s head and the other to a Mosso ergograph which was clamped firmly to a table at the rear. The ergograph recorded upon a smoked kymograph

[3]There were two subjects, Miss H. A. Anderson (*A*) and Dr. C. R. Griffith (*G*), both members of the department.

The chair was selected chiefly to meet the criticism that a Morris chair is relaxing and hence unfavorable to active movements. It is slightly uncomfortable, rather than comfortable. For a brief description see: Griffith, C. R., The Organic Effects of Repeated Bodily Rotation, *Jour. of Exper. Psychol.*, 1920, 3, 20f. and ref.

sheet. After a number of preliminary trials the ergograph was discarded, for it was found to be too insensitive to record very slight involuntary movements. In its place was substituted a Porter[4] lever, which carried a light straw. The lever magnified the actual extent of head movements 7 to 12 times, varying with the straw (which was changed several times during the course of the experiment). As the experiment proceeded the kymograph also was eliminated and graphic records were not taken; it was found that *E* could directly observe and record to better advantage the movements of the pointer. As an aid to observation a white card with black radial lines (about 1 cm. apart, radius 20 cm.) was clamped firmly to a standard behind the pointer. A rubber band, just above the pivot of the lever, kept the tape tight. The position of the pointer upon the scale could be adjusted by moving the standard forward or backward.

The Olfactory Experiment. The following typewritten instruction was used:[5]

"You will be given a series of olfactory stimuli, some of which are *intensive* odors, some have no odors at all. At the signal 'now' smell the stimulus.

"You are asked (1) to report whether the immediate experience is pleasant, unpleasant or indifferent, and to indicate the *intensity of the feeling* (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong'); and (2) to report all muscular tendencies and organic sensations in any way related to the affective reaction."

S was seated, his eyes closed. The stimulus was placed beneath his nostrils 1 cm. or more away. He was permitted to take two and, rarely, three full inhalations. The stimulus was not withdrawn except when there was danger of touching *S*, for it seemed methodologically better to hold the vial in such a position that *S* could move freely toward it or away from it, as the case might be, without definitely suggesting movement by withdrawing the stimulus-object. The apparatus was so delicate that it indicated immediately the slightest movement in either direction. *E* learned to watch the indicator in indirect vision.

The following stimuli were used: oil of mace; stale cheese; oil of juniper; anise; ess. wintergreen; camphor; oil of bergamot; oil of lavender; mutton tallow; heliotrope; heliotropine; oil of lemon; tar water; cloves; nitrobenzol; and a series of eight odors prepared by C. H. Stoelting Co. (sulphuric ether, oil of cloves, oil of peppermint, oil of rose, old fish, carbon disulphide, strong cheese and asafoetida).

[4]The device is a light muscle-lever, with double-hook straw fastener, pictured by W. T. Porter in *An Introduction to Physiology*, 1906, 86. The arrangement proved very satisfactory in that it indicated the slightest movements. Probably these movements could be studied even more minutely by attaching a mirror to *S*'s head, so that a beam of light would be reflected upon the wall or ceiling.

[5]It is identical with the instruction used by Corwin; the last paragraph is practically the same as that of the Minnesota study. *G* asked, somewhat facetiously, how an olfactory stimulus could "have no odor at all!"

To reproduce Corwin's conditions as closely as possible, an electric fan was operated throughout the experimental period. It "served the double function of keeping the air in motion and of eliminating the noise of the kymograph."

TABLE I

Observed movement	P reported Subject		U reported Subject		Indifference Subject		Total
	A	*G*	*A*	*G*	*A*	*G*	
Forward	30	5	2	0	2	4	43
Backward	3	0	23	11	0	1	38
None	4	4	4	2	5	5	24
Forward & backward	1	5	2	5	0	3	16
Total	38	14	31	18	7	13	121

Results. Table 1 shows the gross results. It will be seen immediately that P tends to be associated with forward movement (35 out of 52 cases) and U with backward movement (34 out of 49 cases). Indifference is characterized by no movement (10 out of 20 cases).

With *G* there are three cases of discrepancy between the movements of the pointer as observed and recorded by *E* and the verbal reports of movement processes experienced by *S*. Following are excerpts from these reports:

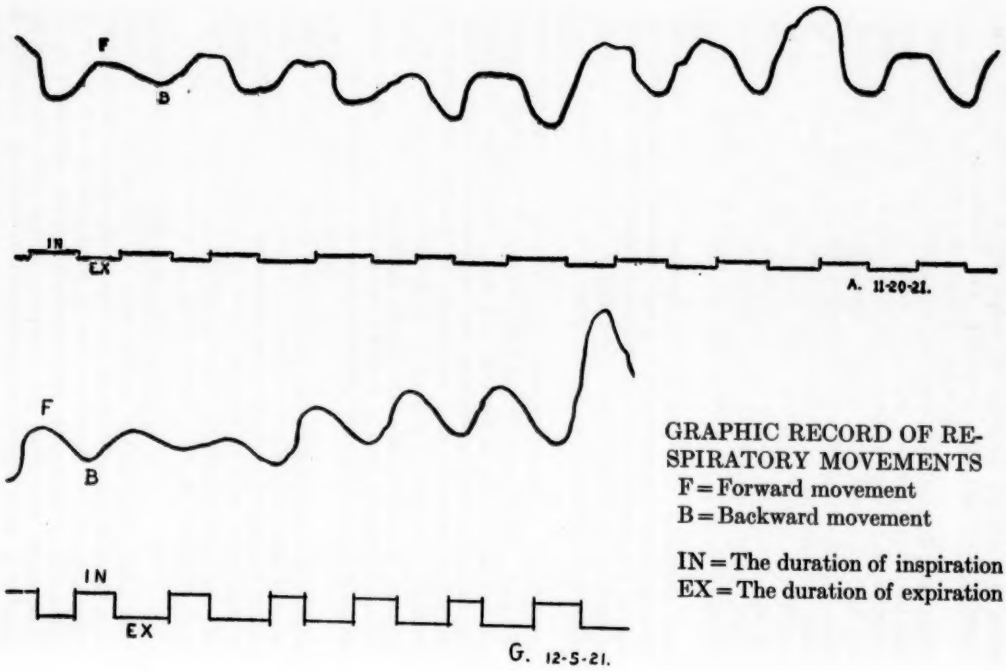
"Decidedly U. Kinaesthesis about mouth and nose and neck as in withdrawal." (Pointer makes a slight movement in the forward direction.) "An exact duplication of the former experience." (The next report; no movement observed.) "Very intense. Extremely U and disgusting. Kinaesthesis about the mouth and face and neck meaning withdrawal." (The pointer makes a gradual and wave-like movement in the forward direction.)

These ambiguous cases have not been included in the data of the table.

The cases in which both forward and backward movements were observed (during the period of stimulation) are usually reports of weak P and U. Following are instances from the data of subject *G*.

"Affectively indifferent." (Slight forward and backward movement.) "Quite P." (Alternate movements in both directions.) "A slight tinge of U." (Movements forward and backward.) "Neither P nor U affectively." (Movements in both directions.) "Faintly U to start with; becoming I as the olfactory quality disappeared." (Slight forward, then backward, then forward movement observed.) "Faintly P." (Forward, backward, forward.) "Rather I." (Forward, then backward movement.) "Very faint; just perceptibly P." (Forward, then backward movement.) "Mildly U becoming very U toward the end." (The pointer makes a slight, gradual forward movement and then a backward.)

One report of *A* was thrown out because it was too complex affectively; the movements of the pointer were complicated and could not be adequately observed by *E*.



GRAPHIC RECORD OF RESPIRATORY MOVEMENTS

F = Forward movement
B = Backward movement

IN = The duration of inspiration
EX = The duration of expiration

From the quantitative data and from the reports of the subjects two conclusions may be drawn. (1) There is probably a tendency for forward movements to occur when *S* reports pleasantness, and also a tendency for backward movements to be associated with unpleasantness. Indifference and weak P and U are characterized by no movement, or else by slight movements in both directions. (2) It is apparent that the correlation is not perfect; in this respect it resembles other correlations gained by the expressive methods.

Breathing Movements. Since every case of smelling is a reaction in which air is inhaled through the nostrils and into the lungs, it is important to know whether normal breathing is accompanied by any characteristic forward or backward movement. To test the matter a number of graphic records were taken on both subjects and also on two individuals not regular subjects in the experiment.

S was instructed to breathe deeply during the test; his eyes were closed. *E* stood to the right of *S* and slightly in front; from this position he could readily observe respiration. At the beginning of every inspiration *E* pressed a hand-key, keeping it pressed throughout the inhalation period; at the beginning of expiration *E* released the key, keeping it released during the period of exhalation. The key was connected to an electric marker which left a tracing upon the smoked paper. The kymograph was not started until breathing seemed normal and regular.

The curves show that inhalation is accompanied by a slight backward movement of the head while expiration is accompanied by a slight forward movement. The changes are purely mechanical and "express" the fact that breathing involves in a particular way the musculature of the chest, shoulders and neck.

For present purposes the significance of these respiratory movements is this: inspiration is accompanied by slight "withdrawing" movements which bear no special relation to the affective phase of experience; and, similarly, mere exhalation is accompanied by "seeking movements." Respiration may account for some of the backward movements associated with affectively weak or indifferent stimuli; and possibly, also, for those instances in which both forward and backward movements were observed; but in these cases the forward movement was regularly observed before the backward, indicating that there are other complicating factors involved.

Pursuit Movements and Attention. (*A*) *Olfactory Reports.* Both subjects report that when the olfactory quality is of weak intensity there are movements toward it in order to get a better smell, a more intensive and a clearer quality. Possibly sensory

adaptation which rapidly reduces the intensity of weak olfactory processes gives rise to "seeking movements" of this sort. At any rate these movements "express" olfactory attention and have at best a doubtful relation to the affective phase of the experience. Following are excerpts to illustrate the point:

Subject A. "P. A tendency toward it; I think that was because it was weak." "P. A slight tendency to go in the direction of the object, but that was because it was weak as much as anything else." "P. A tendency to draw toward it, largely because it is weak." "P. A tendency to draw towards it, partly because it is weak."

Subject G. "Faintly P. Just a little kinaesthesis about the face as if trying to smell to better advantage." "Rather indifferent. A little kinaesthesis about the neck as if leaning forward trying to get a better perception." "A faint tinge of U holding over from the last experience. A bit of kinaesthesis as if trying to get a better smell." "No olfactory quality. The whole experience was indifferent affectively. There may have been a slight change in pressure meaning bending forward to get a better smell." "Rather indifferent. Leaning forward to get a better smell." "Affectively indifferent. A change in kinaesthesis which meant leaning forward to get a better smell." "Perhaps a shade of kinaesthesis which meant leaning forward to get a better smell. Nothing affectively."

In this connection it should be noted that *G* sometimes refers to an 'inviting' character which is not identified with P or U. "It was not P or U; it was rather inviting." "There was a piercing quality that was inviting." "A faint tinge of something rather curiously inviting. I don't know whether there was any positive kinaesthesis of approach or whether it was just the absence of withdrawal." (Slight forward movement recorded.) "Curiously P; rather invitingly P." "Rather invitingly P. The inviting part is carried in kinaesthetic tendencies to move forward. Later it became faintly U and the kinaesthesis that had meant forward movement then dropped out." "Neither P nor U; affectively neutral. Rather inviting; carried in terms of kinaesthesis and those muscular tendencies and the bodily set necessary to get a better smell."

This 'inviting' character is apparently related to approach and the attempt to get a clear perception, but it is not to be confused with P.

(*B*) *The Auditory Experiment.* In view of the result that sensory attention may be accompanied with slight forward movement it seemed advisable to study the involuntary response to an affectively indifferent auditory stimulus. Corwin led phonograph music to the ear of *S* through a rubber tube and then gradually withdrew the tube from the ear. In this way evidence of "seeking" movements was produced; but some of the reports, especially those of *Bi*, indicate that an attentive factor was involved. The present problem is this: what is the normal response to an indifferent auditory stimulus the intensity of which is gradually and continuously decreasing?

A watch was used for stimulation. The following instruction explains the experimental procedure:

"After a 'ready' signal the experimenter will hold a watch near to your ear, and then move it gradually away. Attend to the sounds.

"You are instructed to describe the experience, noting especially kinaesthetic and organic processes."

The subject's chair was turned so that the apparatus lay on a table at the side. The tape was placed around *S*'s head and attached to the indicator. The slightest movement of *S* was immediately apparent to *E*. During the trials *S* sat with eyes closed. Twenty observations were made with each *S*: in the first series, five *R* and then five *L*; in the second, five *L* followed by five *R*. *E*, keeping his eye upon the seconds-hand of the watch, gave the preparatory signals and then placed the watch near the ear of *S*. It was moved away gradually for a distance of about 1 m. and over a period of time which varied from 12 to 25 sec. *E* noted carefully the movements of the pointer and recorded the result as well as the reports of *S*.

In every case, without a single exception for both subjects, there were definite movements toward the stimulus; and the order of magnitude of these movements is the same as that in the olfactory experiment. The movements were found for both ears. The verbal reports confirm the observations of *E*.

A reports as follows. "As the sound became fainter and fainter there was a concentration of attention to it. A tendency to grit the teeth and a tendency to move in the direction of the sound. This seemed a desire to do so rather than a tendency. There was a general tenseness of the body as it became fainter; relaxation when it seemed to get closer and louder." "Gritting of the teeth; closing of the eyes; muscular contraction; tendency to move in the direction of the sound." "Increased tenseness of the body as the sound gets fainter. When it appears again there is relaxation. A tendency (slight and perhaps imaginary) to move in the direction of the sound." "Tenseness about the shoulders, neck, and a slight tendency to move in the direction of the sound." "Turning of the eyes in the direction of the sound. Visual and kinaesthetic imagery of turning the head." "Slight strain sensations about the neck, shoulders, hands. Tendency to hold the breath....A tendency to move in the direction of the sound." "A tendency to move in the direction of the sound; it seemed to be inhibited. A tendency to turn the eyes in the direction of the sound." "A decided tendency to move toward the sound—mostly inhibited. Holding of the breath. The eyes turned toward the sound." "A slight tendency to move toward the sound. There was some strain sensation."

G did not report movement toward the stimulus-object. At the close of the series he was asked whether or not he was aware of any movements. He replied: "Once in a while it seemed as if the kinaesthesis in the neck might be the result of movement but I am not sure. Once I thought that tactual processes from my collar meant movement but again I wasn't sure. These processes are part of the general set to get in the best attitude for hearing. I think there may have been a little movement."

Two types of forward movement were observed. (1) There was a gradual, progressive settling toward the source of stimulation—to the right or left—which continued through the series. This type of movement was indicated by the necessity of readjusting the standard which supported the indicator. The standard had to be moved away from *S* constantly so that the pointer would come to a vertical position before each test. The series of experiments was not long enough to determine how far this gradual and continued "seeking" would

go before coming to an equilibrium.[6] (2) There was an immediate and definite forward movement which commenced shortly after the stimulus was presented and which continued throughout the relatively brief period of stimulation.

II. Discussion of Results

Pursuit and Pleasantness. The experiments under auditory stimulation show conclusively that if *S* merely attends to weak sounds, or to sounds which are becoming intensively weaker, there are movements of approach which indicate sensory attention. These movements, observed when *S* is affectively indifferent, are of the same order of magnitude as those found in the olfactory experiment with P and U odors. Now if pleasant feeling is normally associated with attentive response and unpleasant feeling is accompanied by inhibition of the normal reaction, or by avoidance, or by some other response which may be called inattentive, then attentional pursuit should correlate, in the long run, with P, and its absence or opposite with U. It is extremely doubtful whether the pursuit movements observed in the present experiment express anything more than sensory attention. The actual extent of the movements of *S*'s head is very slight,—usually less than .5 cm. and never more than 2 cm. at the very most. Certainly such movements cannot be interpreted directly and unequivocally as expressions of pleasant feeling.[7]

The failure to find satisfactory evidence of seeking movements which are involuntary expressions of pleasantness is due once again, presumably, to experimental conditions. Corwin stresses the point that there must be an "incentive" to seeking; and her experimental conditions provide adequate incentives. In both the original experiment and the present one this element was lacking. *S*, indeed, was entirely free to move in either direction, but there was no suggestion of movement, unless possibly it were the tape attached to *S*'s head. Had there been some incentive, it is probable that gross seekings would have appeared. Corwin herself states that when she repeated the original experiment under my conditions, *i. e.*, without incentives to seeking, she found no evidence of "definite seeking movements or tendencies to move." If, therefore, some in-

[6] I believe that the same sort of gradual trend can be made out in the experiment with olfactory stimuli. Some of the respiratory curves, also, indicate a gradual adjustment in the forward direction.

[7] This result suggests the conclusion of H. C. Stevens that the "method of expression has failed in the domain of feelings" because of "complications with other mental processes, and.... the psychophysical processes of the sensation." Stevens, H. C., A Plethysmographic Study of Attention, this Journal, 1905, 16, 470.

centive is necessary for the appearance of gross seeking movements, we may well ask: in what sense are gross seeking movements an expression of pleasant feeling?

Comparison of Experiments. A comparison of my two experiments may possibly throw light upon the nature of seeking movements.

Under the original conditions[8] there were 31 cases of P reported as having strong intensity against 27 cases of intense U: under present conditions there are only 3 cases of strong P against 25 strong U. In the original experiment, again, there were reported as many cases of relaxation as of strain:[9] in the present affective experiment (with olfactory stimuli) relaxations are decidedly lacking. *A* reports 3 cases (all in P reports; not one, however, in the *intense* P reports); but *G* does not mention a single case. On the other hand, there is relatively much more straining reported by the *S*s in the present data, *A* alone reporting 21 cases of strain associated with U. A great deal of the straining can be referred definitely to attentive attitudes.[10] Although there are dangers in comparing two sets of data gained under different conditions, nevertheless the following statement is completely warranted by the facts; *viz.*, that under the original conditions there was passivity, relaxation, and moderately intense P feeling, relatively more intense P than U; and under present conditions there is a more active bodily attitude, little or no relaxation, but much strain (especially attentive), and a greatly reduced number of intense P reports relative to the number of intense U. It should be remembered that the present experimental situation, with the iron chair and *S* harnessed to an apparatus, was devised specifically to meet the criticism that the original conditions favored relaxation and passivity and that on this account they were unfavorable to active seeking movements. It turns out (1) that present conditions which presumably favor an active type of response by making bodily relaxation next to impossible are not at all favorable to the development of affectively intense pleasant feelings[11] and (2) that the failure to find valid evidence for pursuit movements as expressions of P is not due to *S*'s comfort or lack of comfort but rather, as Corwin herself remarks, to the lack of a special incentive.

[8] *Op. cit.*, 46.

[9] *Op. cit.*, 40.

[10] It should be noted that *A* reports her strains as contractions and her relaxations as expansions or openings (of the throat). *G* speaks of "muscular contractions" and describes as "kinaesthetic sensations" processes which other *S*s might describe as strain. The distinction between relaxation and expansion, made by Corwin's observers, *may be* valid, but at the present time I am inclined to think that it is largely verbal.

[11] The fact that relaxation favors the development of P feeling does not

III. Critical

Incentives and Seeking Movements. Common instances show that seeking movements are not expressions of pleasant feeling but rather of some incentive or need. (1) Imagine *S* seated comfortably in a Morris chair listening to his favorite piece of music played upon a phonograph. If the selection is loud and clear, he will probably lean back and relax (not a withdrawing movement, however, merely because the phonograph happens to be in front!); he may beat time. In any case the affective experience is ordinarily P. If the music be intensively weak so that *S* would have to 'strain' to hear it, or if *S* had to pursue a receding rubber tube in order to hear the music well, the total affective experience would not be so P as under the first conditions. Hence seeking movements do not express P feeling *as such*; they show, rather, that the conditions for P are not wholly satisfactory in some respect or other. *S* will make movements which are necessary to gain satisfactory conditions or to maintain them, if need be; but the seeking movements are not involuntary expressions of P. (2) In definite and extreme cases of seeking behavior the affective experience is normally U. A drowning rat "seeks" air: a hungry dog "seeks" food: an animal in heat, confined in a cage, "seeks" a mate: a man in a burning house "seeks" safety. These seeking movements express the presence of some need, irritant or incentive rather than P or U; but, if we are bound to associate seeking movements with either of the traditional affective qualities, I submit that definite and extreme seeking behavior is ordinarily accompanied by U. (3) There are also instances of apparent "seeking" which are affectively indifferent. Following an insect or a moving object from curiosity is a case in point. The entire group of attentional "seekings," referred to above, should be mentioned here. (4) Again there are complex and prolonged examples of seeking which involve many changes from P to U or I. Consider a hunting expedition in which a day is spent hunting prey; or consider a life-time spent in seeking a fortune or fame. In these cases there is some

mean that all Ps are essentially or even typically relaxations. There are other types not dealt with in the present series of studies.

There can be no doubt that P is associated with active bodily response in such instances as dancing, beating time to music, free swinging movements (as in certain games), bodily exercises, etc. It also appears that normal uninterrupted physiological functions are frequently, if not normally, accompanied with pleasant feeling. Obvious examples are deglutition and the habituated manipulations which accompany it, coitus, even defecation and urination in some cases, and the entire group of free movements referred to above. In an unpublished experiment upon the affective phase of active movement, two subjects report P associated with free swinging movements of the leg, flexed at the knee.

determination (motive, incentive) which expresses itself in seeking movements, but any attempt to relate the movements themselves to either of the traditional affective qualities is utterly futile.

There can be no doubt that "seeking" movements *may be* associated with P. Corwin's experiment proves the point; but the conditions for such an association involve two factors: (*a*) a weak or receding stimulus, or some other unsatisfactory element in the conditions—a need, irritant, incentive, motive—which makes it necessary for *S* to seek or pursue if he is to react adequately and to maintain conditions under (*b*) stimulation which is normally P. The factors which condition seeking movements are obviously neither necessary nor favorable to the existence of P feeling. The necessity for seeking reduces the intensity of P obtainable and the reduction may go to I or even to U; extreme cases of seeking are U rather than P. If seeking movements express anything, that is an incentive, need, or irritant rather than simple feeling.

It is impossible, therefore, to accept Corwin's interpretation that "the direct response of the organism to P is....a definite activity of pursuit or of tendencies to pursuit." Obviously she has got out of her experiments exactly what she put in. With a little thought it would be possible to devise experimental conditions under which pleasantness would appear to be associated with movements of avoidance.

Sources of Confusion. There is yet another serious objection to the traditional hedonistic doctrine. The reputed correlations between P and seeking and between U and avoidance are both teleological interpretations of behavior. "Seeking" implies an organism purposefully striving for some object or situation which is the aim in view; "avoiding" may have a similar connotation. The teleological nature of these terms is obvious from the following illustrations. Relaxing in a Morris chair while listening to music is "withdrawing" from the phonograph and "seeking" the chair, according to the way in which we interpret the situation. A hunter goes forth to seek a bear; but when the animal is found, perhaps the same man "seeks" safety by "avoiding" the bear. The random incoordinated movements of a drowning rat may be interpreted as "seeking" air or "avoiding" confining conditions. One "seeks" food to "avoid" hunger. Illustrations of this sort could be increased indefinitely to show that "seeking" and "avoiding" are teleological and, moreover, dynamistic interpretations of behavior.[12] The statement that behavior has some purpose or other does not

[12]For the present study these terms were defined in a purely descriptive way to avoid the above implications.

contribute anything to an exact analysis of the facts; it is at best a preliminary statement. Since one can easily read into the same response either the purpose of "seeking" or the purpose of "avoiding," the teleological interpretations lead only to confusion of thought; they do not make for logical clearness.

But there is another source of confusion. Those moralists and biologists who associate P feeling and seeking movements have fallen into an error which I have called 'the meaning error'.[13] To say that one seeks pleasure is not the same as to say that seeking is a bodily accompaniment of P, a mark or expression of pleasantness which is truly felt. What is meant is that one seeks the conditions of pleasure, or seeks to maintain them after they have been obtained. This statement, whether true or false, has little to do with the factual question raised in the present experiment. It may be true, in a sense, that the readiness to seek is the roughest kind of indication of those objects and situations capable of pleasing us; but this is very different from regarding movements of search as typical marks, or expressions, or involuntary characteristics of felt pleasantness. There is no good evidence for the latter belief.

Avoidance and Unpleasantness. It should be noted that, in the case of U and avoidance, there is a solid body of fact which lends itself readily to the traditional interpretation. Characterizing the U response, I have previously noted[14] the "well-known tendency to withdraw oneself from the stimulus-object, either reflexly or deliberately.... (and) the tendency to put the object away from oneself or to prevent its action.... (and the) tendency to inhibit or resist....some normal response to the stimulation...(and finally, the presence of) reflex twitchings, convulsive contractions, and bodily reverberations of various sorts, especially when the stimulus-object is presented suddenly and there is an element of surprise." Neglecting the last mentioned characteristic, it appears that the others have the biological meaning of avoidance, defence or resistance.

The present study, however, indicates that nothing is added to the facts by interpreting these U responses as biological avoidances. As in the case of P and pursuit, there are obvious exceptions to the reputed correlation.[15] It cannot be maintained that all avoiding expresses U. Outstanding instances of the relationship (jerking away the hand when burned, turning the head from a disagreeable sight, holding the breath or vomiting when an U odor is presented, etc.) do not justify a sweeping generalization.

[13] Young, P. T., An Experimental Study of Mixed Feelings, this JOURNAL, 1918, 29, 261f.

[14] *Op. cit.*, 49f.

[15] Suppose, for example, that we define avoidance as getting away from

The direct explanation of "seeking" and "avoiding" behavior may be given ultimately in physiological or chemical terms, but it should be remembered that the mechanisms of response are extremely complex. The central nervous system is highly variable and a thousand factors may intervene between stimulus and response. One factor alone—the experimental instruction, or the self-instruction, of *S*—is capable of turning a "seeking" movement into an "avoidance." Consequently it seems methodologically better to await more information than to read into the facts now at hand some purpose.

Summary

1. In the case of olfactory stimuli of moderate intensity pleasant feeling is sometimes associated with a slight forward movement of the head; and unpleasant feeling, with a slight backward movement. Occasionally no movement at all, or both forward and backward movement, is observed. The reports indicate that forward movement is frequently an indication of olfactory attention. Any interpretation of the data is complicated by the fact that inhalation is accompanied by a backward movement and exhalation by a forward; these respiratory movements are purely mechanical incidents in deep breathing.

2. Attention to a weak and affectively indifferent auditory stimulus, or to one which is decreasing in intensity, is indicated by an involuntary muscular adjustment toward the stimulus. Two types of movement have been noted: (1) a gradual trend toward the source of stimulation which continues throughout the experimental series, and (2) brief and relatively marked movements during the period under controlled observation.

3. Seeking movements, other than those of attention, do not appear as "expressions" of pleasantness unless some incentive is introduced into the conditions. The incentive probably reduces the intensity of feeling obtainable and is not

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an object or situation, or casting it aside. Immediately one thinks of taking off an overcoat and hat. Such behavior has no apparent relation either to P or to U, although it is definitely a matter of casting aside. The next illustration that occurs is a chase in which one animal runs away from another. Are we to assume that the avoiding animal feels only U and that the pursuing animal experiences nothing but P? Again, Professor Bentley has called my attention to the fact that the play of courtship, which may be assumed to be P, is generally regarded as pursuit on the part of the male and as avoidance on the part of the female.

It may not be far-fetched, accepting the above definition, to regard urination and defecation as instances of rejection; and these experiences, according to Boring's observers, may be P, U, or I, varying with the individual. Boring, E. G., Processes Referred to the Alimentary and Urinary Tracts: A Qualitative Analysis, *Psychol. Rev.*, 1915, 22, 323f.

necessary to the existence of pleasantness. Consequently the statement that pleasantness is expressed by involuntary seeking movements is abandoned.

4. A critical analysis shows that seeking movements indicate what might be called a need, irritant, incentive, or determination, rather than simple feeling, and that "seeking" and "avoiding" are teleological interpretations of behavior, and, further, that the reputed correlations involve a confusion between meaning and affective processes. For these reasons it is recommended that the traditional hedonistic doctrine be abandoned and that psychologists study directly the facts of response and of affective psychology.

DIOTIC TONAL VOLUMES AS A FUNCTION OF DIFFERENCE OF PHASE[1]

By H. M. Halverson, University of Maine

The fact that a tone, diotically perceived in the presence of a phase-difference between the two ears, is localized toward the side of the leading phase,[2] suggests that the two stimuli, in integrating to form a single tonal sensation, contribute each a tendency to localization toward their respective sides, and that the stimulus leading in phase, because of its priority, contributes to the spatial aspect of the integration more than does the stimulus that lags. When there is no phase-difference localization is in the median plane: both stimuli contribute equally. When one stimulus leads by half a wave-length localization may be entirely to one side: it would seem as if the tone of prior phase were inhibiting the other tone. To such facts the Bernstein theory of localization with the modifications suggested by Boring[3] seems applicable.[4] On the modified Bernstein hypothesis we should expect median localizations, where displaced excitations unite in a single larger region of excitation, to show a large tonal volume (provided, of course, that it should prove to be correct to equate tonal volume to the analogous attribute of spatial diffusion for the skin); and we should expect lateral localizations, where one stimulus inhibiting the other is singly effective, to show a small volume. The present experiment was undertaken in order to test this hypothesis. The results do not substantiate the hypothesis in any simple manner, since the medianly and laterally localized tones are largest and the intermediate tones smallest. The results are, however, positive and univocal, and are presented here without attempt at a theoretical explanation, but with the conviction that an ultimate theory of auditory localization will have to take account of them.

[1]From the Psychological Laboratory of Clark University.

[2]For the writer's recent papers on this topic and references to the literature, see H. M. Halverson, *Psychol. Monog.*, 1922, no. 140, 7-29; this Journal, 1922, 33, 178-212.

[3]E. G. Boring, *Quart. J. Exp. Physiol.*, 1916, 10, 86-94.

[4]Watt also makes use of the Bernstein theory in his theory of hearing, but in a different manner; H. J. Watt, *Psychology of Sound*, 1917, 175-192; *Brit. J. Psychol.*, 1920, 11, 163-171.

The apparatus was the same as the apparatus for localization with closed tubes described in a previous paper.[5] The source of tone is a 512 d. v. tuning fork, electrically driven by a master-fork. The tone is conducted to the two ears by a pair of rubber tubes which pass through a wall. The phase-relation of the tones of the two tubes is altered by means of a telescopic slide in one of the tubes, which lengthens or shortens the path.

The observers were Mr. M. A. Tinker (*T*), Mr. M. K. Macdonald (*M*), and Mr. F. L. Bixby (*B*), all members of the Psychological Laboratory of Clark University. *T* and *M* were graduate students and trained observers; *B* was a senior of considerable laboratory experience. None of the *O*s, however, had had any previous experience in experimental auditory localization. Their inexperience in localization was considered an advantage in this problem, since the attention was to be to the volumic aspects of the experience and away from its localization.

In order to train the *O*s in volumic judgments and to make sure that their definition of volume had the accepted meaning, a preliminary experiment, patterned after that of Rich[6], was instituted. The *O*s were required to make volumic judgments under the method of constant stimuli upon tones from two Stern variators. The standard was 396 d. v., and the five comparison stimuli ranged from $\frac{1}{8}$ to $\frac{5}{8}$ of a musical tone lower than the standard. At first the *O*s failed to judge consistently. By the end of the second hour, however, *T* and *B* were giving constant results, and by the end of the fourth hour *M* was equally constant. The judgments were not immediate at first, but became immediate as soon as the *O*s had been required to characterize "volume" in a written introspective report. The psychophysical results agree very closely indeed with those of Rich. The average limen for these three *O*s at 396 d. v. is 14.3 d. v. The average limen for Rich's two *O*s at 400 d. v. is 13.4 d. v. Rich's lower limens obtained with tuning-forks[7] are not comparable, since these experiments, like Rich's earlier trials, were with the Stern bottles. The general agreement of these results with Rich's may, however, be taken as an assurance that the *O*s had learned to make judgments of volume as ordinarily defined.

With the *O*s trained to appreciate volumic differences, it was possible to proceed to a preliminary investigation of the volumic relationships of diotic tones where the phase-relationship is varied. Table I gives the results of this preliminary

[5] Halverson, this JOURNAL, 33, 180f.

[6] G. J. Rich, *J. Exp. Psychol.*, 1916, 1, 13-22.

[7] Rich, this JOURNAL, 1919, 30, 149-153.

TABLE I

Preliminary comparison of volumes of diotic tones (512 d. v.; wave-length = 67.3 cm.) for phase-differences of 11.5 cm. (localized 30° left), 24.8 cm. (60° left), and 35 cm. (90° left), compared with zero phase-difference (median localization) as the standard. Observers *T*, *M*, and *B*. Figures are percentages based on 50 judgments for each comparison stimulus.

Localization	30° left			60° left			90° left		
Phase-difference	11.5 cm.			24.8 cm.			35.0 cm.		
Observer	Larger	Equal	Smaller	Larger	Equal	Smaller	Larger	Equal	Smaller
T	0	14	86	0	24	76	16	64	20
M	8	30	62	0	38	62	30	52	18
B	0	32	68	16	34	50	26	56	18
Average	3	25	72	5	32	63	24	57	19

survey. The three phase-differences which give on the average 30°, 60° and 90° left localization were selected for comparison with zero phase-difference, which gives median localization. The table shows that the difference in volume between each of these three comparison stimuli and the standard is not so great as to give univocal judgments. In all three cases the two tones are frequently called "equal," and there are some judgments in the category of difference opposed to the predominant category. Subsequent results (Table II) show, however, that after practice this degree of scatter is reduced. In general it is apparent from Table I that the tone localized at 30° left is predominantly smaller than the medianly localized tone, and that the tone for 60° left is also predominantly smaller, although perhaps not quite so small. For the tone localized laterally at the left, the "equal" judgments predominate; the judgments of "larger" outnumber the judgments of "smaller" for two *O*s, whereas for one *O* the balance is the other way. The lateral tone would thus seem to be of a volume about equal to the volume of the median tone.

The next step is to determine the exact course of the volumic changes, indicated in the preliminary experiment, as phase-difference is altered from zero to the maximum of about a half wave-length, the difference where the tone is localized at 90° left and is about to shift abruptly to 90° right; for a tone leading in phase by more than one-half wave-length is functionally to be regarded as a tone lagging by less than a half wave-length.

The method of procedure was to select zero phase-difference as a standard, and then to determine by the method of constant stimuli the limen for the category "smaller." The

TABLE II

Psychometric functions of volumic differences for phase-differences of diotically perceived tones of 512 d.v. Relative frequencies based on 50 judgments for each comparison stimulus.

Series & standard stimulus (cm.)	Obs.	Range of 5 equally separated comparison stimuli (cm.)	Relative frequencies of volumic difference. I, II, III, "smaller;" IV, V, VI, "larger." Successive comparison stimuli in increasing order of phase-difference					Limen by linear interpolation (cm.)	Average limen (cm.)
			1	2	3	4	5		
I	*T*	6.0–9.0	20	32	52	60	80	7.4	
0.0	*M*	4.0–10.0	20	40	60	74	88	6.3	7.0
	B	5.0–8.0	22	36	44	50	72	7.2	
II	*T*	8.0–12.0	16	38	52	56	68	9.8	
7.0	*M*	8.0–10.0	24	40	66	60	78	8.7	9.2
	B	8.0–10.0	24	24	44	60	78	9.2	
III	*T*	10.0–12.0	28	34	38	52	40	11.5[1]	
9.2	*M*	10.0–12.0	28	38	52	40	52	11.8[2]	11.6
	B	10.0–12.0	18	32	40	46	62	11.6	
IV	*T*	18.0–30.0	10	30	32	62	74	25.8	
11.6	*M*	16.0–24.0	22	36	40	46	68	22.4	24.5
	B	22.0–27.0	20	26	40	56	66	25.3	
V	*T*	28.0–32.0	22	40	52	66	84	29.8	
24.5	*M*	25.5–31.5	26	38	54	70	94	28.1	29.7
	B	27.0–32.0	14	26	34	42	62	31.2	
VI	*T*	31.0–35.0	20	18	28	44	42	[3]	
29.7	*M*	31.0–35.0	24	36	40	50	54	34.0	
	B	33.0–35.0	18	24	30	32	44	[3]	

[1]Minimum of psychometric function from parabola through points 3, 4, and 5. Limen equivocal.
[2]Minimum of psychometric function from least-square parabola through all five points. Limen equivocal.
[3]Limen indeterminate; psychometric function does not include 50%.

*O*s were allowed to report "smaller," "equal," or "larger," but of course no limen for "larger" could be obtained since variation of the stimulus in either direction from the standard tended to give "smaller." When the first set of limens had been obtained (Series I), the limens were averaged for the three *O*s, and this average was used as a new standard for the determination of a new set of limens (Series II). In this manner it was intended to step off the total volumic range by liminal steps. The limit of volumic increase was, however, reached in the third set of limens (Series III). Here the psychometric functions for the *O*s could not be brought to high relative frequencies of the category "smaller;" the minimum volume had been reached, and the functions were reversing direction. Indeed the psychometric functions only barely crossed the 50% abscissa in the cases of *T* and *M*, and returning across it left the exact value of a limen equivocal. For this reason in the column of limens in Table II minima instead of limens are given for these two *O*s. The minimum for *T* was found by passing a parabola through the last three points, and that for *M* by adjusting a parabola by the method of least squares to the entire five points.

Since Series III brought *E* to the volumic minimum, it was necessary for him in Series IV to proceed with limens for the category "larger," just as he had to work only with the category "smaller" when at the maximum in Series I. Series IV was taken with the average of the two minima and one limen of Series III as a standard. Series V was based on the average limen of Series IV, and Series VI on the average limen of Series V. In Series VI maxima were reached, not in the sense that the function again reversed direction, but because the tone now tended to shift to the other side of the head, sometimes existing on both sides at once. This complication would have made careful judgment impossible; moreover it indicated that the limit of variation had been reached, since the tones of the right quadrant simply followed the law of the left. Since the maxima for *T* and *B* in Series VI lay below 50% it is not possible to compute limens for them.

The limens of Table II are all computed by linear interpolation between the two relative frequencies adjacent to 50%. It is obvious from a study of the psychometric functions of Series III and VI that the *phi-gamma* hypothesis is not applicable in those cases. It is also a question whether it should be applied in Series I and IV, which also are limiting cases. Moreover, it is questionable whether any general hypothesis, in the face of the insufficiency of knowledge of the function concerned, could have much value. An hypothesis seeks to take into account remote points as well as the adjacent ones in the

determination of the limen, but it depends for any general validity upon the comparability of different regions of the abscissa scale. In the present case the change of volume for change of phase is irregular; for stretches of the abscissa it seems to change scarcely at all, and then an abrupt change appears. For this reason the mere selection of equal increments of phase-difference for the comparison stimuli gives no assurance that the stimuli are equal in a psychophysical sense and that the remote points can be used to determine the limen. There is no perfectly valid way out of the difficulty. The observed fact, assuming continuity of function, is that the limen lies between the stimulus values for the two relative frequencies that include 50%. If one seeks to say just where, one may follow the convention of linear interpolation as readily as any more difficult assumption. As a matter of fact, with unusual psychometric functions of this sort, the limen has very little meaning, and attention has to be paid to the form and nature of the entire function.

In Fig. 1 the data of Table II are shown graphically in such a way as to indicate the general form of the volumic function. Against difference of phase are plotted for each *O* the six psychometric functions of the six successive series. The ordinate scale for each psychometric function is so arranged that its zero comes opposite the limen of the preceding psychometric function. The average limen for any series was taken as the standard for the succeeding series, and the zero for the scale may be placed at the frequency that the standard had in the preceding series, on the assumption that the standard in the new series would give 0% difference with itself (a rough but not perfect assumption, since a time-error may exist). In this manner it is possible to plot off the volumic function with liminal steps as the ordinate unit and roughly to fill in the breaks between the six observed liminal points by plotting the psychometric functions for these points. The total constellation of the grouped psychometric functions of Fig. 1 gives as closely as possible on the basis of these results the form and amount of the volumic function in general.

Inspection of the three charts of Fig. 1 shows striking agreement among *O*s. It is possible to state the form of the function in general for all *O*s.

As phase-difference increases from zero, the decrease in volume of the diotic tone is at first gradual; and then from about $\pi/8$ to $3\pi/8$ there is an abrupt and rapid decrease to the minimum volume, which is about three liminal steps smaller than the volume for the tones in phase. After $3\pi/8$ there appears to be a gradual increase up to a phase-difference of about $3\pi/4$; thereafter the increase in volume is rapid although not

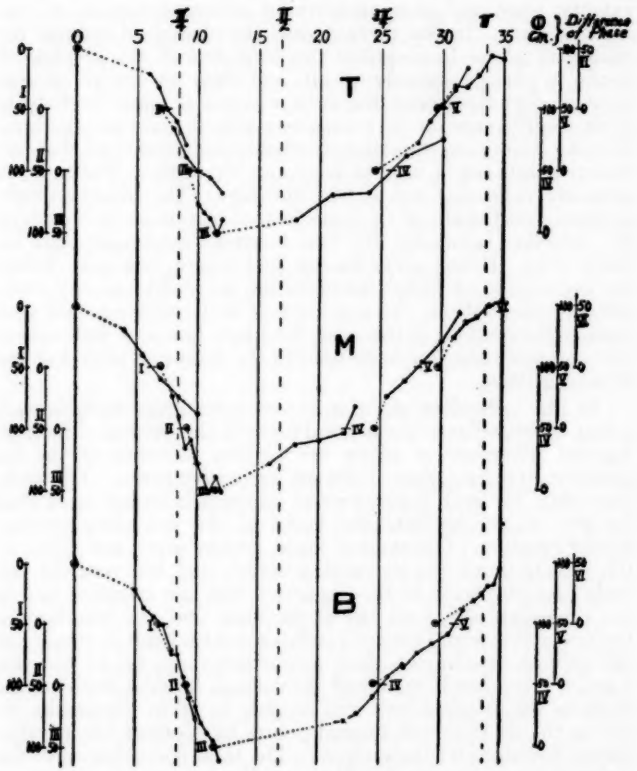


Fig. 1. Volume of diotically perceived tones as a function of difference of phase. Observers *T*, *M*, and *B*. Tones of 512 d. v.; wave-length = 67.3 cm. Abscissa is phase-difference of tones between the two ears, in cm. and wave-length. The psychometric functions for smaller volumes (Series I, II, and III), and for larger volumes (Series IV, V, and VI) are shown successively so scaled that 0% for one function corresponds to the limen of the preceding function, for the reason that the average limen of one function was always used as the standard for the next. For this reason the dotted lines are added on the rough assumption that the standard would give 0% difference from itself as comparison stimulus. The total constellation of the psychometric functions for each *O* gives approximately the form of the function that volume, measured by liminal units (ordinates), is of phase-difference (abscissa).

so rapid as the preceding decrease. The volume reaches a maximum at π where it about equals the volume for zero difference of phase. This last result of Fig. 1 accords with the preliminary results of Table I, which also indicated that the median and laterally localized tones are of about the same volume.

Some *Os* in the localization of diotic tones have described the image of localization as getting farther away from the head when it passes from the median position of localization, and then coming in close to the head when the position of lateral localization is being reached. This change of distance in the image accords with the volumic function; the smaller volumes, it would seem, are perceived as further distant.

The question still remains whether these volumic functions truly represent a distinct volumic aspect of the tonal sensation. There are two other obvious possibilities; the judgments might be dependent entirely or in part upon pitch-difference or upon intensive differences.

The most direct source of information is the *Os*' reports. They consistently described the tones as spatial and their judgments as indicating the spatial aspect. For them the tones are "full," "fat," "lean," "tridimensional," "large," "small," "homogeneously filled," "big," "thin," "extensive," "crowding in from all sides."

The well known association of volumic change with change of pitch and the fact that these *Os* were trained to observe change of volume with change of pitch in the practice-series may nevertheless raise the question whether pitch-differences could be present here and lie at the basis of the volumic function. It is difficult, however, to see how an effective difference of pitch could occur. The lengthening of the one tube with respect to the other might lower the pitch at one side. In such a case there should be binaural beats, but no beats were heard. Moreover a continuous increase of friction would not account for the reversal of the change in the volumic function. It seemed important, nevertheless, to test the matter directly, and the *Os* were asked to make discriminations of pitch between the diotic tone of zero phase-difference and the tone reported as of smallest volume. Both *T* and *M* were unable to detect difference of pitch in a series of over 25 paired presentations. Their results were supported by Dr. Pratt, who has a small limen for pitch discrimination. *B* tended to report differences of pitch; but his capacity for pitch discrimination is known on the basis of tests to be low. With this pair of tones he reported the smaller as higher 9 times, the larger as higher 11 times, and the two as equal 9 times. There is therefore no evidence of a discriminable difference in pitch between the two tones which

give the maximal volumic difference. Similar negative results were obtained from comparing the smallest tone with the lateral tone, and also from comparing the median tone and the lateral tone.

There remains the matter of the relation of volume to intensity. There is no doubt that the diotic tones varied intensively, and casual observation makes it appear as if the intensive function might be similar in form to the volumic. All *O*s mentioned the presence of intensive differences in their protocols. Occasionally they thought that their judgments might have been influenced by intensive differences, but at other times they were positive that they had not been so influenced. For them to have been on their guard against intensity as the basis of judgment gives more assurance that it was not the basis than if they had not mentioned it. On the other hand, it is doubtful whether under these conditions intensity can be separated from volume as was pitch. It is not impossible that the two are essentially covariant. The Bernstein hypothesis, which was responsible for the undertaking of this problem, would imply such a relationship, and, although these experiments have not brought out the function that a modified Bernstein theory implied, they do not disprove the relationship between extensity and intensity that is the basis of that theory. It may be that experiments would show that volume varies with the intensity of a tone in diotic hearing without phase-difference.

Conclusion

Diotically perceived tones vary in their volumic aspect when the phase relationship between them is altered. Medianly localized tones, where the phase-difference is zero, and laterally localized tones, where the phase-difference is half the wave-length, are largest. The intermediate phase-differences give smaller tones, and the minimal volume occurs for a difference in phase of about 3/16 wave-length. These volumic changes are of the same kind as the changes of volume that accompany changes of pitch. They are unaccompanied by changes of pitch under these conditions, but are accompanied by changes of intensity.

AN EXPERIMENTAL STUDY OF HENNING'S SYSTEM OF OLFACTORY QUALITIES[1]

By MALCOLM K. MACDONALD, University of Pennsylvania

The purpose of this study was the verification of the qualitative olfactory continuum represented by Henning's[2] smell prism. We hoped either to verify the general nature of the continuum or to discover necessary modifications of it. As secondary matters we hoped that we might be able to specify a practical set of stimuli for the demonstration of the prism and, by using stimuli of known chemical nature where possible, to obtain some incidental information bearing upon Henning's theory of the chemistry of the olfactory stimulus.

Henning represents the odors as lying in a qualitative continuum represented by the surface of a triangular prism.[3] At the six corners of this prism are the six principal groups of odors, which correspond to the *Hauptfarben* of the color pyramid as fundamental points of reference. At the corners of one triangular face of the prism lie the fragrant (F), ethereal (E), and putrid (P) odors, and at the other end respectively opposite these three are the spicy (S), resinous (R), and burned (B) odors. Along the edges and in the five faces lie psychologically simple and qualitatively intermediate odors, but the interior of the prism is hollow.

It will be convenient if we designate the principal odors at the corners as *simplex* odors. Such odors are the points of reference for the rest of the figure; they are fundamental and cannot be described qualitatively, whereas the other odors can be described by reference to them. Odors lying along an edge may be called *duplex* for the reason that they can be described by their qualitative similarity to two simplex odors. They are of course simple and not complex, but are defined in the qualitative system in a duplex manner. They may be designated as FE, EP, PF, SR, RB, BS, FS, ER, or PB. Within the faces there would be *multiplex* odors. At the middle of the rectangular sides (Henning's *Quadrupelpunkte*[4]) the multiplex odors are quadruplex: FESR, EPRB, and FPSB. In strict logic there should be no triplex odors in the rectangular sides, since any odor here should have fourfold similarity. Henning, however, appears at times to consider that odors near one corner and remote from the diagonal are practically triplex: dehydrocamphylcarbinol appears to be FSR[5].

[1]From the Psychological Laboratory of Clark University. Communicated by Edwin G. Boring.

[2]H. Henning, *Der Geruch*, Leipzig, 1916, pp. 533. Reprinted from the *Z. f. Psychol.*, 1915, 73, 161-257(=pp. 1-97 of *Der Geruch*); 1916, 74, 305-443 (=pp. 98-226); 1916, 75, 177-230 (=pp. 227-280); 1916, 76, 1-27 (=pp. 281-407). *Cf.* E. A. McC. Gamble, *Psychol. Bull.*, 1916, 13, 135 ff.; this JOURNAL, 1921, 32, 290 ff.

[3]Henning, 80-97.

[4]Henning, 96, 299.

[5]Henning, 299.

The triangular ends should of course contain triplex odors, but Henning does not discuss these faces. This terminology is not Henning's, but is useful in representing his system.

Henning has a chemical theory[6] of the olfactory stimulus, which, if it held rigorously, would be a valuable aid in determining the stimuli for different regions of the prism. Odor, according to this theory, depends primarily upon the structure of the molecule of the stimulus, although it is also to some extent dependent upon the nature of the component elements.

The stimuli to the *simplex odors* have a single structural characteristic. The stimuli for the FESR face are all theoretically derivatives of the benzine ring. The PB line is characterized by the introduction of certain other elements as well as by molecular structure. The simplex stimuli are as follows.

Fragrant. An *ortho*-substitution product of the benzine ring, *i. e.*, substitution of chemical groups for the hydrogen at two adjacent corners of the six corners of the ring.

Spicy. A *para*-substitution product of the benzine ring, *i. e.*, substitution of chemical groups for hydrogen at two opposite corners of the ring.

Ortho-substitution and para-substitution are both forms of disubstitution. The only other disubstitution is meta-substitution, where the substitution is at the corners next but one. The meta-substitutions do not enter into the theory and should presumably be odorless. Monosubstitutions similarly do not appear in the theory. In a disubstitution product it makes no difference at which pair of corners the disubstitution occurs, provided only the spatial relation is fixed: all para-substitutions are identical for the same substituted groups, but different from ortho-substitution for the same substituted groups.

Resinous. An *inner* substitution in the benzine ring, *i. e.*, a disubstitution of a single group linking across between two corners of the ring and usually represented as within the ring.

Ethereal. A *hydrocarbon forked chain*, not necessarily related to the benzine ring, but discussed by Henning as if it belonged with the benzine derivatives. The theory is complicated at this point by the fact that there are few, if any, pure ethereal stimuli; nearly all are duplex ES and combine in the stimulus the fork with the para-substitution in the ring.

Putrid. Forked chains in which *sulphur, selenium, tellurium, nitrogen, phosphorus, arsenic, antimony, bismuth* are osmophores (smell producers; analogous to chromophores). Henning describes these forks as a disruption of the ring, but there seems to be no obvious chemical ground for this interpretation of a ring.

Burned. Heterocyclic rings including *nitrogen* with various numbers of sides. The benzine ring is homocyclic, since carbon is always the link, and it always has six sides. In a B stimulus nitrogen is substituted for carbon at one corner (not for hydrogen, as in the benzine derivatives).

The *duplex odors* have, on this theory, duplex stimuli, *i. e.*, stimuli that combine two of the simplex chemical characteristics. Thus in the FESR face they tend to be trisubstitution products. The stimulus to an FS odor combines the ortho- and para-substitution of the F and S groups. Such a ring is known as an asymmetrical ring. The stimulus to an SR odor combines inner and para-substitution; the FE stimulus should include a fork in one of the ortho-substitutions; and the ER stimulus should have a fork added to the inner substitution. There ought, on the theory, to be no FR odors which are not also E and S, and no ES odors which are not also F and R; and the corresponding duplex stimuli ought not to

[6]Henning, 281-305, esp. 291-301.

occur. Unfortunately for the theory, ES odors, as Henning admits, are the rule rather than the exception, and they are given by stimuli that add a fork to a para-substitution.

The duplex stimuli for the two faces which include the P and B corners are not well worked out by Henning, and the general principle of the theory cannot be extended here.

The faces should contain *quadruplex* stimuli, and possibly triplex stimuli, if there are any strictly triplex odors. The FESR face does contain both triplex and quadruplex stimuli. Thujone, the stimulus to the quadruplex odor at the center, illustrates the theory, for it is an asymmetrical ring (ortho- and para-substitution) with an additional inner substitution and with the para- connection carrying a fork.

The foregoing terms for the six principal olfactory groups have the advantage that they have different initials and provide a simple terminology for both simplex and multiplex odors (as do R, Y, G, O, RO, YG, *etc.*, for the colors). The usual English words and Henning's German which they translate are as follows.

F = Fragrant (flowery, *blumig, duftend*);
E = Ethereal (fruity, *früchtig*);
P = Putrid (foul, *faulig*);
S = Spicy (aromatic, *würzig, gewürzhaft*);
R = Resinous (piny, balsamy, *harzig, balsamisch*);
B = Burned (burning, scorched, empyreumatic, *brenzlich*).

Stimuli[7]

We sought to select stimuli which should represent all parts of the prism and which have been definitely placed by Henning. In addition we sought, when possible, to choose stimuli of definite chemical composition in order that we might obtain some incidental information concerning Henning's theory of the stimulus, and also for the reason that such a stimulus is more definitely defined in nature than is a common object. The selection proved peculiarly difficult, because Henning skimps many parts of the prism in his lists of representative odors. In some regions, where Henning cites definite chemical examples, we were unable to obtain the stimuli. For example, we did not succeed in finding thujone (FESR), camphene (ER), cadaverine (PB), chavicol (S), cineol (R), or cajuputol (R), in this country or in England. The PB edge does not permit of laboratory investigation; Henning had to take his *Os* to the zoological gardens to obtain these odors.

Table I gives a list of the stimuli which were used at any time during the experiment. A more definite characterization of these stimuli, and a statement of the reason for the rejection of many, follow. After the name of each substance is given its position on the prism according to Henning and in parentheses the page-reference to the statement in *Der Geruch*. It will be observed that, of these 35 stimuli, 20 are chemicals and 15 objects.

[7]Our thanks are due to Dr. George F. White, Professor of Organic Chemistry at Clark University, for his assistance in the selection and procuring of our stimuli and in obtaining information concerning their chemical structure.

TABLE I

LIST OF STIMULI. Standards are given in italics. An asterisk indicates a stimulus of definite chemical structure.

1	F	*Ionone
2	E	*Acetic ether
3	FS	*Vanillin
4	FESR	*Menthol
5	PB	*Amyl alcohol
6	RB	Burned mastic
7	B	Tar
8	B	**Pyridine*
9	B	*Nicotine
10	P	*Carbon disulphide
11	ER	*Xylene
12	SB	Burned coffee
13	P	*Asparagine
14	S	*Anisol
15	P	**Thiophenol*
16	R	Frankincense
17	FPSB	Celery
18	S	Nutmeg
19	R	Balsam of Peru
20	FESR	Wormwood
21	EPRB	Grapefruit rind
22	EP	Rotten fruit
23	FPSB	Onion
24	F	*Oil of jasmine*
25	FS	Vanilla
26	FE	*Citronellol
27	R	**Eucalyptol*
28	FPSB	*Apiol
29	S	**Anethol*
30	FE	*Geraniol
31	E	**Citral*
32	FP	Rotten flowers
33	SR	Allspice
34	SR	*Myrtenol
35	R	*Pinene

SIMPLEX STIMULI

F	1	*Ionone
	24	*Oil of jasmine*
E	2	*Acetic ether
	31	**Citral*
P	10	*Carbon disulphide
	13	*Asparagine
	15	**Thiophenol*
S	14	*Anisol
	18	Nutmeg
	29	**Anethol*
R	16	Frankincense
	19	Balsam of Peru
	27	**Eucalyptol*
	35	*Pinene
B	7	Tar
	8	**Pyridine*
	9	*Nicotine

DUPLEX STIMULI

FE	26	*Citronellol
	30	*Geraniol
EP	22	Rotten fruit
FP	32	Rotten flowers
SR	33	Allspice
	34	*Myrtenol
RB	6	Burned mastic
SB	12	Burned coffee
FS	3	*Vanillin
	25	Vanilla
ER	11	*Xylene
PB	5	*Amyl alcohol

MULTIPLEX STIMULI

FESR	4	*Menthol
	20	Wormwood
EPRB	21	Grapefruit rind
FPSB	17	Celery
	23	Onion
	28	*Apiol

Simplex Stimuli

1. Ionone. Not mentioned by Henning. Synthetic violet; might be expected to belong to the F corner. It is a derivative of tetrahydro-benzine and a vicinal ring, which is equivalent to two ortho-substitutions, and thus may be said to accord with Henning's theory. Considered as a possible standard for the F corner, but rejected in favor of oil of jasmine which has a stronger odor. Used throughout the experiment.

24. Oil of jasmine. F. (H. 82) Used throughout the experiment as a standard for the F corner.

2. Acetic ether. E. (H. 83) Henning places all the ethers at this corner of the prism. Acetic ether involves the forked structure required by Henning's theory. Used throughout the experiment.

31. Citral. E. (H. 295) Used as a standard for the E corner throughout the experiment. It is an open chain with a fork, and readily condenses into a ring with para-substitution and a fork. Thus it approximately fits Henning's theory. Its structure would seem to imply that it should be ES (or FESR?) and not pure E. (*Cf.* no. 26.)

10. Carbon disulphide. P. (H. 301) Considered as a possible standard for the P corner, but rejected because its odor is said to be due to impurities. As an open chain involving sulphur it should, on Henning's theory, lie at the P corner. Used throughout Part I.

13. Asparagine. P. (H. 301) Occurs in the form of crystals as an open chain involving nitrogen. It had no perceptible odor for our *Os*, in spite of the fact that Henning gives it as typical of the P quality. The crystal was loaned us and we were not permitted to try dissolving it. Used only in the very first part of the experiment.

15. Thiophenol (*Merkaptane*). P. (H. 301) The aromatic mercaptan used was thiophenol. As a mono-substitution of a sulphur chain in benzine it is a proper P on Henning's theory. As obtained from the chemical laboratory it was so strong as quickly to permeate the whole room when the bottle was opened. The method hit upon was to hold an empty bottle over the mouth of the bottle containing thiophenol. In a few minutes enough of the stimulus had adhered to the inside of the bottle to last for some time, but the process had to be repeated often. Used throughout Part I as a standard for the P corner.

14. Anisol. S. (H. 292) A mono-substitution product of benzine; its para-derivatives Henning characterizes as S. In the shortened series it was eliminated in favor of nutmeg, which has a definitely spicy quality.

18. Nutmeg (*Muskat*). S. (H. 81) Whole nutmegs were grated as needed. Used throughout the experiment.

29. Anethol. S. (H. 292) Satisfactory S stimulus, and used as a standard for the S corner throughout the experiment. It is a para-substitution product as required by Henning's theory.

16. Frankincense (*Olibanum, Weihrauch*). R. (H. 84) Weak stimulus but strengthened by pulverizing. Used throughout the experiment.

19. Balsam of Peru. R. (H. 84) Eliminated because too weak a stimulus.

27. Eucalyptol. R. (H. 297, *cf.* 84) Satisfactory representative of the R corner and used as a standard for that corner. An inner ring as required by Henning's theory.

35. Pinene. R. (H. 297) A good chemical stimulus for the R corner. It accords with Henning's chemical theory, since it is an inner ring. Used throughout the experiment.

7. Tar. B. (H. 85) Coal tar was found to have only a faint odor, which was, however, strong enough when the tar was pulverized. Used throughout Part I.

8. Pyridine. B. (H. 85, 300) As pure pyridine was too strong, it was diluted with distilled water. Pyridine is a 'smooth' heterocyclic nitrogen

ring, and thus represents Henning's typical B both qualitatively and chemically. Hence it was used as a standard for the B corner throughout Part I.

9. Nicotine. B. (H. 300) Similar to pyridine both chemically and in Henning's qualitative classification. Hence should be satisfactory, but proved too weak to give good results. Used throughout Part I.

Duplex Stimuli

26. Citronellol. FE. (H. 296) Eliminated in favor of geraniol (no. 30) in order to reduce the number of stimuli on the FE line. It is an open chain with a fork and readily condenses into a ring with para-substitution and a fork, which should presumably under Henning's theory make it ES (or FESR?) instead of FE.

30. Geraniol. FE. (H. 296) Satisfactory FE stimulus, and used throughout the experiment. It is an open chain with a fork and readily condenses into a ring with para-substitution and a fork. It is therefore like citronellol and raises, because of its chemistry, the same theoretical objection. (*Cf.* no. 26.)

22. Rotten fruit. EP. Not mentioned by Henning, although seemingly implied as an intermediate. Early eliminated, however, because even when very rotten it was mostly like E. Rotten apples and oranges were used. It was difficult to keep this stimulus constant.

32. Carrion flowers (*Aaspflanze*). FP. (H. 85) The carrion flower was not available, but we used decayed flowers. They were, however, open to the same objection as decayed fruit (*cf.* no. 22) and were therefore eliminated.

33. Allspice. SR. (*Cf.* H. 34) Ground allspice. Used throughout the experiment as presumably representative of the SR line.

34. Myrtenol. SR. (H. 298) According to Henning a good representative of the SR line since it combines the inner and para-substitutions in the benzine ring; other texts, however, cite it as an oil of uncertain composition. Used throughout the experiment.

6. Burned mastic (*brennendes Mastixharz*). RB. (H. 85) The mastic gum on being burned forms a solid mass which has no perceptible odor. Discarded as an impractical stimulus. At best its odor is very weak.

12. Burned coffee (*gebrannter Kaffee*). BS. (H. 85) This was an unsatisfactory stimulus, for coffee exposed soon loses its odor and burning or roasting does not renew it. Eliminated during the first series.

3. Vanillin. FS. (H. 83) An asymmetrical ring and thus in accordance with Henning's theory. Used throughout the experiment.

25. Vanilla. FS. (H. 83) Vanilla beans. In the later series vanilla was eliminated in favor of the chemical vanillin (no. 3) which has a definite composition.

11. Xylene. ER. (H. 85) A disubstitution of CH_3 for H in a benzine ring. Does not seem to fit the chemical theory. Used throughout the experiment.

5. Amyl alcohol. PB. (H. 85) An open chain alcohol (a hydrogen, carbon and oxygen compound) with a fork, and should therefore on Henning's theory lie at the E corner and not along this edge. Used throughout Part I.

Multiplex Stimuli

4. Menthol. FESR. (H. 85) Henning places it between E and S, and it would seem, therefore, from the logic of the prism, to be also between F and R, although it is not given as a quadruplex stimulus (H. 96). Chemically, on Henning's theory, it should be a triplex (FES) stimulus, since it combines the para-(S), ortho-(F), and forked (E) structures. Used throughout the experiment.

20. Wormwood (*Wermutarten*). FESR. (H. 96) Unfortunately had to be eliminated because a very weak stimulus, although it is, according to Henning, the typical quadruplex stimulus of this face of the prism. Menthol (*v.* no. 4) is theoretically a representative of the interior of this face.

21. Grapefruit rind (*Pompelmusfrucht*). EPRB. (H. 96) Henning mentions grapefruit for the middle of the EPRB face. New rind had to be obtained frequently, as it decayed rapidly. Used throughout Part I.

17. Celery. FPSB. (H.85, 96) Celery seed. Celery according to Henning represents the quadruplex stimulus for this face of the prism. Used throughout Part I.

23. Onion. FPSB. (H. 85) Eliminated because celery (no. 17) represents the same place on the prism and is a more constant stimulus.

28. Apiol. FPSB. (H. 96) Satisfactory as a stimulus, and used throughout Part I. It is a complex penta-substitution product, apparently not related to Henning's chemistry of the prism.

Procedure. Henning insists on dirhinic smelling.[8] To ensure the use of both nostrils, 50 cc. glass-stopper, salt-mouth bottles (6.34 cm. deep, 2.54 cm. mouth-diameter) were used for presenting the stimuli. Henning also states that memory-smells, akin to memory-colors, fuse with the quality when the *Os* know the nature of the substances.[9] To pay regard to this admonition, the bottles were covered with paper and the *Os* were instructed not to look within. The different stimuli were identified by inconspicuous numbers on the covering of the bottles. The numbers had no relation to the position of the odors on the prism.

Standards. As a standard for the F-corner there was a choice among several of the essential perfume oils, oil of jasmine, oil of geranium, oil of ylang-ylang, and tonka bean, oppoponax, mimosa and coumarin. We selected *oil of jasmine*, which was of sufficient strength for a satisfactory standard, after rejecting the chemical ionone (*v. supra*).

For the E-corner there was a choice of the several ethereal oils, such as oil of lemon peel, oil of lime, oil of orange, as well as some of the ethers, *e. g.*, acetic ether. *Citral* was chosen as a standard on account of its known chemical nature as against the oils, and because it has a stronger and more distinct odor than the ethers.

For the P-corner there was the choice of carbon disulphide, hydrogen sulphide, the mercaptans, the cacodyls, and the stabines. Carbon disulphide was not used because its odor is said to be due merely to the presence of impurities. *Thiophenol*, one of the mercaptans, was selected because of its distinct strength of odor.

For the S-corner, from the ordinary spices and several chemicals, *anethol* was chosen on account of known chemical structure and availability.

Eucalyptol was chosen for the R-corner, from among frankincense, oil of cedarwood, tincture of myrrh, *etc.*, because of its known chemical structure.

From a number of chemicals at the B-corner, *pyridine* was chosen because sufficiently strong and easily available.

The standards thus selected were covered with paper, as were the other stimuli, but were prominently marked with large black gummed letters from I to N. These letters were chosen as having fewer associations than those at the beginning or end of the alphabet, and as having no relation to the names of the Henning classes or of the substances. The translation from the arbitrary symbol to the Henning class-symbol is as follows: I = F, J = E, K = P, L = S, M = R, and N = B.

[8] Henning, 11-17.

[9] Henning, 26-39.

Observers

The *O*s who took part in this experiment were *H*, Dr. H. M. Halverson; *Ba*, Dr. Marjory Bates; and *Mo*, Miss E. F. Möller, all generally trained in introspection upon qualitative processes. They all had complete dirhinic olfaction. Another *O* was rejected because of monorhinic olfaction, due presumably to partial obstruction of one of the nasal passages. His inability to make satisfactory judgments in the preliminary experiments supports Henning's statement that dirhinic smelling is necessary for the complete appreciation of olfactory quality.

I. Test of the Prism by a Method of Paired Comparisons

The first method of experimentation was a form of paired comparisons, modified to suit the implications of the Henning prism. Every stimulus was presented to the *O* with every different pair of the six standards. He was allowed to smell the three stimuli in any order and as much and as often as he desired, and was instructed to report which of the two standards the comparison stimulus resembled more closely in olfactory quality. Half-hour periods of experimentation were used to avoid excessive adaptation. The frequency of the selection of the standards was computed for each stimulus; 'equal' judgments were counted one-half for each of two standards.

Table II shows the average and m.v. of nine series, three sets of observations with each of three *O*s. It is apparent from inspection of the table that variability is great and that no absolute verification of the prism was achieved. The original data show that this variability occurred both between *O*s and between the three series of a single *O*. In fact it was the large amount of variability that determined us to combine all series for all *O*s in the hope of finding some general tendency among observations which should support Henning's theory.

In examining the table it should be borne in mind that the maximal rank under a method of paired comparisons with six paired standards is 5: the ranks for a single stimulus, unless there are tie cases, should be 5, 4, 3, 2, 1, 0. The averages, however, show no such wide scatter of ranks; the scores are reduced and brought closer together by the large variability included in the average. Thus also the table shows that the m.v.'s are large with respect to the average ranks. If the results expressed in the table were perfectly self-consistent, the m. v. in every case would be 0; if they were inconsistent with maximum perversity the m. v. could be as high as 2.5. The average m. v. for the entire table (108 cases) actually is 1.12. This means, at any rate, that *O*s accustomed to introspective observation upon qualitative processes, and presumably indisposed to objectification of odors, could nevertheless not give judgments consistent with themselves or with one another when the task consisted in relating unknown stimuli qualitatively to the corners of the supposed prism. At least we may assert that the prism is by no means so easy to establish observationally as is the color pyramid.

TABLE II

Average ranks of the six principal qualities when their stimuli are compared by a method of paired comparisons with every one of the 18 comparison stimuli. The average is the average of 3 series for each of 3 *Os*, 9 values in all; it measures the relative degree of qualitative similarity to the quality named at the top of the column. The highest possible average rank would be 5, since there are 5 comparison pairs which include any one standard. The m. v. shows the variability; the maximum m. v. would be 2.5.

Comparison No.	Stimulus Class	STANDARD STIMULI F av.	F m.v.	E av.	E m.v.	P av.	P m.v.	S av.	S m.v.	R av.	R m.v.	B av.	B m.v.
1	F	2.5	2.1	2.4	1.6	.9	.9	2.5	.9	1.0	1.6	.7	.8
2	E	3.6	.7	3.7	1.0	1.0	.5	2.7	1.0	1.2	1.0	1.2	.8
10	P	0.0	0.0	0.0	0.0	3.5	1.2	1.1	1.4	.1	.2	3.5	1.5
18	S	2.8	1.3	3.5	.8	.8	1.0	3.2	1.5	0.0	0.0	.3	.6
16	R	.7	1.0	3.6	1.2	1.4	1.5	2.0	1.3	2.1	1.7	.9	1.0
35	R	1.3	1.5	2.0	1.8	1.5	1.1	1.3	1.4	3.1	1.6	1.5	1.3
7	B	.9	1.0	2.0	1.4	1.8	1.6	2.0	1.6	1.8	2.0	.9	.5
9	B	1.1	1.4	.8	1.0	2.3	1.6	1.7	1.8	.4	.6	1.9	1.7
30	FE	3.2	1.2	4.1	.4	.9	2.2	3.5	.8	.2	.3	1.1	1.0
33	SR	2.9	1.0	2.6	1.9	.9	1.2	3.4	1.0	.2	3.4	.4	.7
34	SR	.8	.9	2.0	1.4	1.3	1.2	2.3	1.9	3.1	1.6	2.0	1.5
3	FS	4.4	.5	2.8	1.2	.1	.1	3.6	1.2	0.0	0.0	.6	1.0
11	ER	.6	.6	1.6	1.5	3.7	1.1	1.8	1.5	2.2	1.2	2.4	1.5
5	PB	2.7	1.3	2.1	1.2	.7	1.0	2.7	1.4	3.7	1.4	.4	.6
4	FESR	3.1	.9	3.1	.7	.5	.8	3.2	.8	2.5	1.6	.4	.4
21	EPRB	3.7	.6	4.8	.2	.4	.6	2.5	1.1	.2	.3	.6	.8
17	FPSB	2.0	1.7	1.5	1.5	1.2	1.5	2.3	1.2	0.0	0.0	1.5	1.6
28	FPSB	.3	.5	1.2	1.4	3.2	1.6	2.4	1.8	1.0	1.2	1.5	1.3
Av. m.v.			1.01		1.12		1.14		1.31		1.09		1.03

It is interesting to note that the rank order of variability for the principal qualitative groups is, from the most to the least consistent, F, B, R, E, P, S. The low rank of E and S is undoubtedly due to the fact that the two groups, as Henning points out,[10] are related. Most fruits are also spicy in odor, and the forked structure is apt to be added to a para-ring. Hence actually the supposedly E stimuli are not pure E and may deviate in some unpredictable manner toward S. Similarly the S's under our method of experimentation may deviate toward E, for the reason that citral, the standard for E, approximates an ES stimulus. This defect lies in the Henning theory; we could not with assurance choose a better standard for E. Consistency is high for F and R, for these groups are more definitely establishable. Introspection shows F and R to be quite different in quality, and they are never confused as are E and S. In general, the PB portion of the prism is unsatisfactory because too few stimuli can be found adequately to represent it, and the positions of P and B in the rank order given above are less significant than those for the other four groups.

The variabilty, as shown in Table II, seems not to be due to lack of practice within the limits of our experiment, for the third series shows just as little agreement among *Os* as did the first series.

In the face of such great variability the natural resort is to statistical procedure. We have therefore correlated the theoretical ranks, which would obtain if the three sides of the Henning prism are squares, with the observed ranks.

It will be seen that the theoretical ranks are somewhat different for simplex, duplex and multiplex stimuli. For a simplex stimulus the quality for the corner at which it lies should rank 5; the three corners, equally distant from this corner by the side of a square, should each rank 3, and the two remaining corners, distant by the diagonal of the square, should rank 0.5 each. With a duplex stimulus the two adjacent corners should rank 4.5 each, and the four remaining corners 1.5 each. With a quadruplex stimulus the four equidistant corners should rank 3.5 each; and the two remaining corners should receive no score at all, for the reason that the prism is hollow and there is no single continuum leading to the opposite edge whereby a judgment of similarity can be made. Theoretically the ideal series of ranks would be:

For a simplex stimulus: 5, 3, 3, 3, 0.5, 0.5.
For a duplex stimulus: 4.5, 4.5, 1.5, 1.5, 1.5, 1.5.
For a quadruplex stimulus: 3.5, 3.5, 3.5, 3.5, 0, 0.

The coefficients of correlation (products-moments) between these theoretical and the observed values are as follows:

Standard Stimulus:	F	E	P	S	R	B
r=	.360	.432	.309	.334	.435	.119
P.E.$_r$ =	.138	.129	.144	.141	.129	.157

The average correlation is r= .331. The low value for B (r= .119) is explained by the fact that a potentially B stimulus tended to yield to all the other five groups in judged similarity; *cf.* Table II.

[10]Henning, 295.

In general, then, we may say that we succeeded in verifying the Henning prism only to the extent of agreement that is indicated by a coefficient of correlation of 30% to 40%. We shall see later some of the difficulties which accounted for this rather unexpected failure.

II. Test of the FESR Face by a Method of Qualitative Placement

Since the PB line and its adjacent faces could be only incompletely filled in, we decided to confine our further experimentation to the FESR face, for which numerous representatives were available.

The *Os* were trained to make judgments of qualitative relationship in the following manner. A card 15 inches on a side was divided into 25 three-inch squares. Twenty-five small colored squares about one inch on a side, all between red and blue and of various tints, a systematic selection from the Milton-Bradley series, were cut from colored papers. A light red, a dark red, a light blue and a dark blue were placed at the four corners of the square, thus indicating a red-blue bidimensional qualitative continuum in which the hue varied in one dimension and the tint in the other. The *Os* were asked to place, separately, each one of the other 21 colors in the continuum in qualitative relationship to the four standards at the corners. They found no difficulty in performing this task accurately.

These conditions were now duplicated for the FESR face of Henning's prism. The F, E, S, and R standards were substituted for the colors at the corners of the large square, and the *Os* were asked to place singly on the square every one of the 11 olfactory stimuli of the FESR group in qualitative relation to the standards. A series consisted of the placement of each of the 11 stimuli. Ten such series were taken, each at a separate session. The results are shown in Table III and in Figg. 1-3. In the figures the circles show the position of average placement for such stimuli as the *O* was able to place as many as five times out of 10 trials, and the lines run from the positions of actual placement to the average.

All the *Os* experienced difficulty in making the placements, a difficulty which they did not have in the preliminary trial with the colors. They responded differently, however. *Mo* accepted the instruction and managed every time to find a place for every stimulus; *Ba* found a place for only three of the 11 stimuli five or more times. *Ba* was never well satisfied with her judgments and worked under protest. No stimulus did she place more than seven times out of 10; every stimulus she placed at least once. Altogether she made 36 placements out of a possible 110. *H* placed eight stimuli five or more times and made 76 placements out of 110.

TABLE III

Average placement of 11 stimuli by 3 *Os* (*Ba*, *H*, *Mo*) in the FESR face of the prism. x = horizontal displacement from the FS edge toward the ER edge; y = vertical displacement from the SR edge towards the FE edge. All figures are in units defined by length of side = 10 units, *i. e.*, the maximal horizontal or vertical displacement is 10 and the maximal diagonal displacement is 14.14. Average displacement is the average deviation of the placements from the average position shown in Figg. 1-3. Ten trials for each stimulus were given each *O*, but *H* and *Ba* did not always succeed in making a placement within the square. When the number of placements is less than 5, no averages have been computed.

Comparison Stimulus		No. Cases			Average x			Average y			Average Displacement		
No.	Class	*Ba*	*H*	*Mo*	*Ba*	*H*	*Mo*	*Ba*	*H*	*Mo*	*Ba*	*H*	*Mo*
1	F	6	10	10	2.50	8.92	3.25	6.66	9.91	7.25	2.83	1.27	2.23
2	E	1	10	10	—	7.25	5.50	—	8.25	5.50	—	2.71	2.13
3	FS	3	4	10	—	—	3.00	—	—	8.50	—	—	1.79
4	FESR	3	10	10	—	5.75	4.75	—	4.25	4.66	—	1.79	2.04
11	ER	1	1	10	—	—	7.00	—	—	4.25	—	—	2.21
16	R	7	0	10	3.58	—	4.75	4.25	—	8.00	3.07	—	1.50
18	S	2	9	10	—	0.58	3.50	—	3.00	4.25	—	1.52	2.17
30	FE	3	10	10	—	3.75	5.25	—	4.75	5.05	—	2.71	1.06
33	SR	2	5	10	—	0.50	4.25	—	4.00	4.50	—	1.54	2.25
34	SR	5	9	10	6.50	10.00	7.75	2.50	3.33	6.25	2.46	1.56	1.38
35	R	3	8	10	—	10.00	6.00	—	1.58	5.50	—	1.14	3.06
								Average			2.82	1.70	1.98

The figures and Table III show that the *Os* varied considerably. *H* was the most consistent *O*: on the average his displacement of a stimulus from its average position was 1.70, where the length of the side of the square is 10. *Mo* is the next most consistent, with an average displacement of 1.98. Her greater variability may be due to her acceptance of the instruction in such a way as to lead her to place all the stimuli. *Ba*, however, who refused to make placements because of her lack of assurance, is still less consistent for the three bottles which she placed 5, 6, and 7 times; her average displacement is 2.82.

We took protocols from all the *Os* concerning their procedure in making placements. *H* and *Mo* both reported that they began by selecting the one of the four standards that most resembled the comparison stimulus. Then they found the standard or standards which next most resembled the comparison stimulus, and placed it with respect to all the standards that had come under consideration, correcting the placement with respect to the remaining standard or standards later. *Mo*'s procedure was more fixed than *H*'s. After deciding upon the first standard, she always went on to place the comparison stimulus in relation to the two adjacent standards, ending with a final correction which took account of the standard diagonally opposite. *Ba* gave little account of method, since she was more concerned with the rejection of stimuli than with their placement.

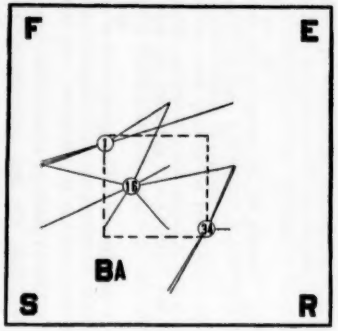


FIG. 1

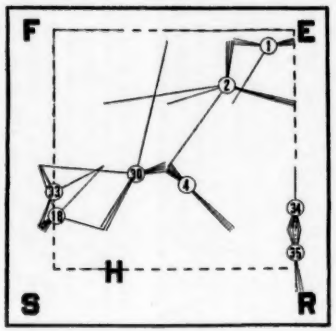


FIG. 2

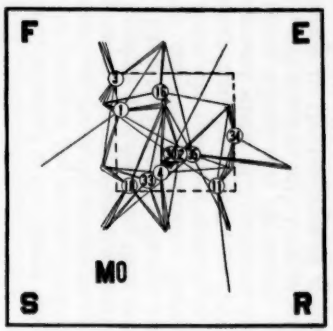


FIG. 3

FIGS. 1-3. Average position and displacements, indicated by radial lines, of the stimuli which *Ba*, *H*, and *Mo* placed in the FESR face of the prism five or more times out of ten trials. See Tables I and III. The dotted lines indicate the "reduced squares" used in computing the average deviation from theory. The key to the stimulus numbers and their classification is as follows:

1	F	Ionone	4	FESR	Menthol	18	S	Nutmeg	34	SR	Myrtenol
2	E	Acetic ether	11	ER	Xylene	30	FE	Geraniol	35	R	Pinene
3	FS	Vanillin	16	R	Frankincense	33	SR	Allspice			

All three *Os* had a great deal to say of the dfficulty of the task. Resemblance was not apparent, as with the colors, and the odors sometimes seemed complex.

After the completion of the main series with the FESR face of the prism, we conducted a supplementary series in which each *O* was asked to place every stimulus once and to give a full account of the method of placement.

These supplementary protocols accorded in general method of placement with the accounts given during the main series mentioned above. In addition, however, they brought out certain typical difficulties of the task which bear directly on the nature of the prism. These difficulties may be summarized as follows.

(1) Occasionally a stimulus is reported as resembling none of the four standards and is placed off the square. (No. 33, allspice, for *Ba*; no. 35, pinene, for *Mo*.)

(2) Frequently stimuli resemble a standard and some other unknown quality that does not lie in the square. Such qualities belong to some extension of the continuum beyond the square, and are properly placed off the square. (No. 3, vanillin, for *Ba* and *H*; no. 4, menthol, for *Ba*; no. 11, xylene, for *H*; no. 16, frankincense, for *H*; no. 30, geraniol, for *Ba*; no. 33, allspice, for *H* and *Mo*; no. 35, pinene, for *Ba* and *H*.)

(3) Occasionally a triplex quality is reported as about equally similar to three standards but not resembling the fourth. Strictly speaking, such a quality can not be placed in the square, since its placement with respect to three corners immediately implies a relationship to the fourth. As a matter of fact *Mo* placed such stimuli near the center of the square in spite of the incorrect implication. (No. 2, acetic ether, for *Mo*; no. 30, geraniol, for *Mo*.)

(4) Sometimes there is reported a duplex quality resembilng diagonally opposite standards. Such a quality also ought not to be placed, since its location near the midpoint of a diagonal puts it also at the midpoint of the other diagonal and renders it a quadruplex quality. In three of these cases the stimuli were actually, though thus incorrectly, placed near the middle of the square; in one case *Ba* refused to make a placement because of the incompatibility involved. (No. 1, ionone, reported ES by *Ba* and not placed; no. 4, menthol, reported FR by *H*, although Henning calls it ES; no. 13, nutmeg, reported ES by *Mo*; no. 30, geraniol, reported ES by *H*.) It is to be recalled that Henning especially notes the existence of the duplex ES, although he does not explain the inconsistency that it implies in the prism.

It will be noted in the figures that the placements tend toward the center of the square. Various factors may account for this result. (1) Since the placements were bounded by the edges of the square, chance placement would tend toward the middle. (2) The odors were noted for their dissimilarity from the standards. Even on Henning's theory, two F's, for example, may be very different sensations. Since the four standards only were given, the tendency must have been to place the stimuli well away from the corners. (3) It is possible that the method of placement which *Mo* employed led her to put most of her comparison stimuli near the center. By invariably bringing the stimuli into relation with three standards she tended to favor placements in the positions of multiplex stimuli and to discourage placements as duplex and simplex odors. In examining

the figures, then, we must take into account the tendency toward the center and attend more to the relative placement than to the actual position in the given FESR square.

We may now examine the results for each of the stimuli separately, noting the stimulus number, the substance, the Henning classification, and the *O*s who placed it five or more times.

1. Ionone. F. *Ba, H, Mo.* Chemically it should be a good F. *Ba* and *Mo* place it toward F in their constellations, but *H* consistently approximates it to E.

30. Geraniol. FE. *H, Mo.* Henning classifies it as FE. Chemically however (see above) it should be ES, which under the logic of the square continuum should become FESR. Both *O*s localize it in the center, as its chemistry on Henning's assumptions would require.

2. Acetic ether. E. *H, Mo.* Chemically also it should be E. *H* places it near E and *Mo* places it near the center; *H*'s greatest deviations are, however, toward the center and *Mo*'s toward E.

11. Xylene. ER. *Mo.* Supposedly ER, but does not accord with the chemical theory (see above). *Mo* places it consistently near the ER side of her constellation.

16. Frankincense. R. *Ba, Mo. Ba* makes it central and *Mo* places it toward the FE side. It does not seem to be R. It was, however, a weak stimulus and less satisfactory on this account than others.

35. Pinene. R. *H, Mo.* Also belongs chemically at R. *H* places it at R with great consistency, but *Mo* places it near the center.

33. Allspice. SR. *H, Mo.* We assumed that it should be SR from Henning's account of this line. It is probably to be considered an S since it is close to no. 18.

34. Myrtenol. SR. *Ba, H, Mo.* According to Henning this is also chemically SR, although we have not been able to verify his formula. *H*, however, makes it definitely ER; *Mo* locates it toward ER; and *Ba* puts it as near ER as SR.

18. Nutmeg. S. *H, Mo.* Both *O*s place it toward the S corner.

3. Vanillin. FS. *Mo.* She makes it F rather than FS.

4. Menthol. FESR. *H, Mo.* According to Henning it is a duplex ES; according to its chemistry it should be triplex FES; according to the logic of the square continuum it would have to be FESR if it were ES. Both *O*s conform to Henning's statement by placing it near the center. It does not seem to tend toward the F side, however.

It is difficult to make any general statement of the agreement of these results with Henning's theory. So many factors enter in, and agreement can occur or fail in so many ways. It is, however, possible to measure the deviation of the average position from the theoretical place for each stimulus, but in so doing we encounter the difficulty that arises from the tendency of all *O*s to group the placements in the center of the square. We have shown the possible causes for this tendency, and it does not seem fair to include it as a factor in any measure of the relationship between observation and theory. Accordingly we undertook to correct for it by redrawing the squares of Figg. 1-3 so that they would just include all the average points (see dotted squares in the figures) and computing the average deviation, for each *O*, of the average placement from the theoretical position. If the side of the reduced square be taken as 10 units,

then these average displacements in terms of such units are as follows: for *Ba*, 4.85; for *H*, 4.23; for *Mo*, 5.35. In other words, the discrepancy between theory and actual placement is on the average about one-half the length of a side of the square for every *O*. It does not appear, therefore, that agreement with theory is very much greater in these series with the FESR face than the correlation of 30-40% that we found before for the entire prism.

Logical and Factual Inadequacies of Henning's Theory

1. The prismatic theory of odor may be said in general still to be insufficiently worked out. For certain portions of the prism Henning gives numerous examples that should enable one more or less readily to reconstruct the qualitative system in experience, but in other portions a definite knowledge of the necessary stimuli is still lacking.

Thus a discussion of the internal constitution of the triangular bases of the prism is entirely lacking; presumably triplex odors should lie here.

Moreover, stimuli for the two faces which include the PB line are insufficient. Henning himself noted the difficulty of filling out the PB line, but the adjacent faces are also inadequately represented. We obtained no satisfactory stimuli for the FP, EP, SB, and RB lines, and Henning gives no definite chemical substances that belong to these positions. With respect to the quadruplex odors belonging to these faces, we find that FPSB can be adequately represented by the celery lactones, the onion esters, and the parsley phenols, of which apiol is an example; but the EPRB face has only the indefinite odor of grapefruit to represent it.

Another insufficiency appears at the E corner. Most of the fruity odors, which belong to this group, are actually similar to S, so that they are not proper representatives of E. Henning's own statement, the chemistry of these odors according to Henning's theory, and our own qualitative findings accord here.

2. Henning's chemical theory also presents insufficiencies in the FPSB and EPRB faces. It is not made clear how chemical intermediates between P and B, on the one hand, and F, E, S, and R, on the other, can occur or what the nature of an intermediate would be. The lack of examples makes it impossible to deduce generalizations. There is no apparent chemical reason why apiol, for example, a complex penta-substitution product, should be a quadruplex FPSB stimulus.

3. The question arises whether the FESR face is properly a square. The close relationship of the E and S corners suggests that the ES diagonal may be shorter than the FR diagonal. It may be, however, that the face is theoretically a square and

that the best practicable stimuli for E are ES, with pure E lying diagonally out beyond, very much as the corners of the theoretical color triangle lie beyond the actual spectral triangle enclosed within it.

4. The existence of triplex odors in a face seems to constitute a logical difficulty for the prismatic theory, in which the odors on a line should be duplex and the odors in a face quadruplex. Henning, however, seems to admit the existence of triplex odors (*e. g.*, thymol, p. 299, and dehydrocamphylcarbinol. p. 299), and our *O*s also report triplex odors which they have for this reason difficulty in placing upon the square (*e. g.*, acetic ether, FES, and geraniol, FER, for *Mo*).

5. An even more fundamental logical difficulty with the prism lies in the fact that there seem to be duplex odors for the diagonally opposite corners. Henning especially notes the usualness of the ES odors, and our observer *H* reported menthol as FR. If the prism is to stand, it is absolutely necessary that ES stimuli should also be FR and conversely, for only quadruplex odors could lie at the center of the diagonals. The trouble seems to be with the prism and not with Henning's observations; our *O*s frequently reported ES odors.

The solution may lie in some other geometrical construction If a solid tetrahedron were substituted for the FESR face, it would be possible to have all duplex, triplex and quadruplex odors that could lie between these four corners. If the other faces were similar, we might have a hollow hyper-solid with solid tetrahedrons as its sides. There is no reason why mental continua should occur only under Euclidian limitations.

6. This same difficulty with the ES odors appears also on the side of the stimulus. ES stimuli are normal, for the forked structure is found attached to the para-substitution. If Henning's theory were to be taken strictly there should be no such stimuli nor combinations of inner with ortho-rings (FR); the combination of one pair of these characteristics should necessitate the addition of the other pair. Geraniol, citral, and citronellol all violate the logic of the prism, in that they combine para-substitution with a fork without further additions.

7. Similarly, there should be no chemically triplex stimuli, since the position of the odor of such a stimulus in a face implies a similarity to the fourth corner. The case of menthol illustrates both this difficulty and the one preceding. Henning describes menthol as ES, a duplex odor, and *H* called it duplex FR. Chemically, it combines the ortho- and para-substitution with a forked structure, and should be a triplex stimulus, FES. Logically, however, since it lies in a face, it should be a quadruplex FESR.

8. A general difficulty with the Henning theory lies in the fact that it seeks to explain a qualitative continuum by correlation with discrete chemical changes. From S to F, for example, there is only one possible chemical intermediate, the asymmetrical ring that combines the para- and ortho-substitution, whereas there are apparently many qualities, with perhaps the possibility of continuous qualitative change.

9. In basing the FESR face on the benzine ring, it must be noted that Henning is interpreting the nature of the E stimuli rather broadly. These molecules, bearing a forked structure, are open chains and not rings. Their relation to the benzine group lies only in the fact that they may readily condense into rings.

The molecules of the P stimuli Henning describes as disrupted rings (*Aufsplitterung der Ringe*), although they are open chains without relation to the cyclic molecules. It would seem that the emphasis should be placed, as we have had occasion to do above, upon the nature of the osmophores and not upon the molecular structure.

10. The chemical theory does not permit rigorous prediction of quality, if Henning's qualitative descriptions are accepted. Among our own stimuli the following inconsistencies appeared. Citral, described as E by Henning, should be ES on the basis of its molecular structure. Citronellol and geraniol Henning seems to characterize as FE: their structure demands that they be ES: they include no ortho-substitution products that would make them F. Menthol, as we have seen, is called ES by Henning, when chemically it is FES. Amyl alcohol is an open chain with a fork and belongs chemically at E, but it is one of the few examples besides the animal foetors that Henning places on the PB line. Xylene is a disubstitution product and might be expected to be F or S; it is placed by Henning on the ER line. Carbon disulphide has a proper formula to represent the P corner, but its odor is said to be due to impurities.

11. Criticism of the chemical theory has necessarily been based on the internal consistency of Henning's own presentation. Criticism of the prismatic theory of olfactory quality is the main purpose of the present study. In it we have shown by a method of paired comparisons, involving stimuli that should represent the entire prism, that we could obtain only 30-40% correlation between the qualitative relationships as stated by Henning and as observed by our *O*s. We have also shown that, in placing stimuli within the FESR face, our *O*s, even when allowance is made for their tendency to place all unknowns toward the center of the square, deviated from Henning's placement by an amount equal to about one-half the side of the square. Both these results, however, issue from data

where individual variation is large, so that it may be argued that our failure better to verify the theory may be due to some fundamental difficulty in observing the qualitative relationships implied. The existence of such difficulties was apparent in certain series where the *O*s found some unknowns related to qualities not represented in the system and (rarely) to no qualities in the system at all.

It is the opinion of the writer that Henning's theories represent an advance, a first approximation to the truth. They can not, however, be applied rigorously, nor does the smell prism represent so definitely understood a system of qualities as does the color pyramid.

Note on Dimmick's Experiment

Since the acceptance of this manuscript for publication Dimmick has published "A Note on Henning's Smell Series."[10] Our proof sheets do not afford an adequate opportunity for the comparison of Dimmick's experiment with ours, but subsequent discussion may be avoided if certain divergences between the two experiments are noted here.

Both experiments are prejudiced to some extent by necessary methodical presuppositions. Dimmick assumed the validity of the prismatic theory of quality and sought to determine the best or most constant representatives of the theory; his study can not constitute a critique of the theory, but has the advantage of determining the best principal odors under the theory. Our study, on the other hand, raises the question of the validity and degree of applicability of the prismatic theory, but necessarily had to assume specific stimuli as representative of the principal classes of odor.

Our effort to obtain standards of definite chemical composition renders comparison difficult. Dimmick justifies our F standard (oil of jasmine), and raises a presumption in favor of the appropriateness of our E and R standards (if citrol and eucalyptol can be regarded as equivalent stimuli to lemon oil and eucalyptus oil). We are unable to come in contact with Dimmick's experiment at all with respect to the S and P corners. Pyridine, our standard for B, Dimmick places on the PB line.

There are only seven stimuli identical to the two experiments, and six more which are similar, as, *e.g.*, anisol and anise oil.

There seem to be a few instances (we have noted six) where Dimmick and we differ in the interpretation of Henning's specification of a stimulus. These discrepancies, however, are, with one exception, never greater than the difference between a corner and a line leading to that corner; and in these matters Henning is remarkably difficult of interpretation.

In general we note that the greatest difficulty in the test of the theory consists in the difficulty of establishing in advance the principal classes of odors to which the unknowns are to be referred in the qualitative schema. Given R, Y, G, B, Wh, and Bk, it is easy enough to demonstrate the validity of the color pyramid, but there seems to be as yet, in spite of these two experiments, no similarly easy mode of determining the principal olfactory points of reference. *Hauptrot*, for example, may be difficult of exact determination, but the general concept of "redness" can be easily be given to an *O*, much more readily than can the concept of "fragrance" or a *hauptblumig* odor. Moreover, it would seem that the various F's are very much more different from one another than is any group of colors all of which could be called "red."

[10] F. L. Dimmick, this Journal, 1922, 33, 423-425.

A STUDY OF LIMINAL SOUND INTENSITIES AND THE APPLICATION OF WEBER'S LAW TO TONES OF DIFFERENT PITCH

By Martha Guernsey, University of Michigan

That the normal human ear is not uniformly sensitive to all gradations in the tonal scale has become a recognized physical fact. The absolute limit of that sensitivity, however, and a measure of the absolute intensity required to elicit response to different pitches are factors which, so far, have never been wholly determined. Very slow and very fast vibrations we either do not hear or else hear uncertainly; and the character of these turning points, *i.e.*, whether they are sudden disappearances or gradual declines, as well as the possibility of varied sensitivity to pitches within these boundaries, have proved an interesting problem both from a physical and a psychological point of view.

In reviewing the literature on auditory phenomena, we find that the bulk of sound experiments comprises such phases as pitch discrimination, the much mooted question of tonal attributes, and physiological theories, with a very meagre contribution in the nature of quantitative measurements of intensity or the audible limits. The material in the following table (A), taken with permission of Professor Pillsbury from the *Psych. Review Mon. Supp.*, 13, 1911, and supplemented with an addition from Gildemeister, summarizes practically the whole of the available quantitative data.

Many of the earlier psychological investigations of sound were carried on with freely falling balls and pendulums; but these apparatus tended rather toward controversy over physical formulae than toward adequate conclusions. For his measurements of sound intensity, Lord Rayleigh used metal cans with vibrations induced through an electromagnet. His results are larger than Wien's, and are limited to determinations of just two pitches in the lower middle range. Toeppler and Boltzmann utilized closed tubes, in which the concentration of sound was measured by a method of interference.

Tuning forks, both electrically and simply driven, have furnished a considerable bulk of experimental data. They were employed in the investigations of Zwaardemaker and Quix, Wead, Stumpf, and by some of the more recent experimenters, all of whose results nevertheless refuse to harmonize on the ground of a common apparatus. This disparity may be due to

TABLE A

N	*WN.*	*R.*	*ZW.& Q*	*WD.*	*T.&B.*	*G.*
32						Upper limit (15000–20000 vd.)
50	4.10^{-6}					0.2–0.5 watt
64						
96			$2{,}8.10^{-1}$			
100	7.10^{-9}					
128			$2{,}7.10^{-1}$			
181					3.10^{-6}	
192			$4{,}6.10^{-1}$			
200	3.10^{-11}					
256		$8{,}5.10^{-9}$	$5{,}5.10^{-2}$	83.10^{-6}		
384		6.10^{-9}	$3{,}4.10^{-2}$	28.10^{-7}		
400	3.10^{-14}					
512			$1{,}97.10^{-3}$			
768			$2{,}5.10^{-4}$	31.10^{-7}		
800	7.10^{-15}					
1024			$2{,}7.10^{-4}$	11.10^{-7}		
1536				22.10^{-6}		
1600	1.10^{-15}					
2048				71.10^{-7}		
3200	5.10^{-16}					
6400	3.10^{-15}					
12800	5.10^{-14}					

The values in the first column represent the rate of vibration of the various tones. The succeeding columns include the energy in ergs required for minimal intensities of these tones as determined respectively by Wien, Rayleigh, Zwaardemaker and Quix, Wead,[1] Toeppler and Boltzmann, and Gildemeister.

[1]Wead's measurements represent the energy in the tuning fork itself. The energy affecting the ear would be much less.

the difficulties inherent in the tuning forks themselves, or perhaps to differences in the psychophysical methods of acquiring results.

A recent experiment dealing primarily with the upper audible limit, but affording also its quantitative equivalent in energy, is that of Gildemeister. His apparatus utilizes a condenser and induction coils, and has many factors in common with the apparatus we employ in our work at Michigan.

Weiss has also investigated the sound-intensity reaction, using electrically driven forks and shifting resonators. His aim is not so much to determine actual limens as to secure evidence for his physiological theory; but some of his results correlate with pertinent phases of Weber's law. He found that when the resonators are near the fork the just-noticeable differences are shorter steps than when the resonators are farther away, and that this increase in length is approximately a logarithmic series. Weiss found also a rather marked disparity between individual discriminations within the same "critical range."

In reviewing apparatus, a mention of the transmitter of Wente and the thermophone of Arnold and Crandall should not be omitted, although pertinent results from their work are not yet available.

It is Wien's determinations of liminal sensitivity, however, which are of the most relevant interest here. His work, in fact, affords probably the most authoritative, and certainly the most comprehensive reference for tones of weak audibility; and his apparatus, based on the theory that the energy of the increasing stimulus serves directly as a measure of the mass of the sensation, has provided a workable model for use in our own telephone set-up. He has left results, however, only for tones ranging from 50 vd. to 13,000 vd., and the validity of these is somewhat lessened by the fact that he apparently used only himself as subject, thus omitting the factor of individual differences in auditory acuity.

As the manuscript is finished the articles of Bunch and Zuehl appear in the University of Iowa studies. They used a method of inducing the pitch by varying the rate of rotation of an armature near electromagnets. The results are given only relatively in terms of the resistance required to reduce to zero the tone produced by a standard current. This procedure neglects the fact that the intensity of the sound varies as the square of the rate of vibration as well as with the amplitude. The rapid falling-off in sensitivity above 750 vd., which Zuehl's curves show as compared with our measurements and Wien's, is to be explained by this fact.

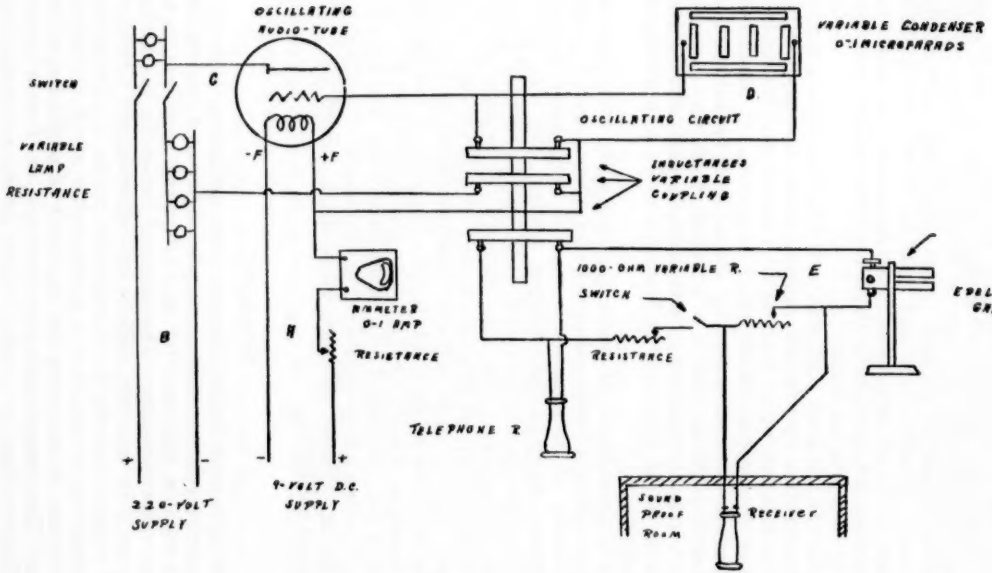
The Helmholtz resonance theory favors a hypothesis of sudden turning points of intensity in the tonal scale. Wien has challenged this assumption by a series of experiments which, though maintaining definite turning points, makes them rather gradual, and recurring through several octaves. In general, Wien found that the curve of liminal intensity in hearing increases markedly from the lowest audible tones to those of 3,000 vd., where it begins to diminish, at first slowly, then more rapidly. The region between 1000 and 5000 vd. elicits the greatest sensitivity. These latter results are apparently substantiated by the work of Gildemeister, though only one definite measurement is offered in his article.

From the foregoing brief review, it will be seen that the apparatus hitherto used for determining sound intensity roughly classifies into five main divisions: (1) use of pendulums, falling balls, etc.; (2) use of discs and resonators; (3) use of tuning forks and electromagnets; (4) use of direct optical methods for observing the motions of a vibrating diaphragm; and (5) use of telephone transmitters with subsidiary apparatus. Our apparatus in the laboratory at Michigan uses a combination of the last two.

All of the methods, of course, presuppose the same physical and physiological factors of sound phenomena: namely, that the intensity of the sensation depends in rough measure upon the kinetic energy of the vibrating medium, and that at the same time the ear, in reporting it, is affected by subjective conditions of perception. The same intensity, for instance, will appear to be different for different *O*s, and even for the same *O*s on different days or under different experimental conditions, and this factor has added decidedly to the complexity of establishing rigid measures of absolute intensity. Our experiment has consequently been conducted on a quantitative basis, and has consisted of taking a large number of measurements of minimal intensities at different vibration rates, using trained *O*s under controlled conditions.

Apparatus

Our apparatus utilizes vacuum-tube oscillation, thus providing a novel method of obtaining pitch differences by electrical tuning such as that used for high frequency amplification in wireless communication. The principal factors in the tuning circuit are a vacuum tube, coils for self-induction, and a condenser of variable capacity for changing the pitch. To the induction circuit is also attached a galvanometer which provides a direct means of measuring the amplitude of oscillation of the current. Two current sources are employed, one from a battery of sufficient voltage (9–12 volts) to heat the filament of the tube,



APPARATUS PRODUCING LIMINAL INTENSITIES.

the other from a power source of 220 volts d.c. The grid is in series with one coil and the condenser; the plate, with a second coil. We thus have two currents which, through mutual induction, start each other into oscillations at a rate determined by the amount of self-induction and capacity in the circuit.

The telephone circuit is composed of a third coil, the telephone receiver, a known variable resistance, and an Edelmann string galvanometer. The last instrument is substituted for the dynamometer of Wien, with the probable advantage of increased delicacy and accuracy. Calibrated to linear measurements, a single scale division is equivalent to 1/242 mm. vibration of the plate.

The different pitches are established by varying the capacity, and these resulting tones are measured by comparing them with Edelmann forks and Galton whistles. For higher tones, ranging from 4,000 to 13,650 vd., we use smaller inductance coils with higher frequencies, and the frequencies are computed from capacity and inductance. In the table of results those tones which were determined directly are indicated by *d*. Those which were computed are preceded by *c*. The method of computation for the higher tones is relatively simple, employing the known measurements of capacity and inductance in the formula $f = \frac{1}{2\pi\sqrt{LC}}$. We could determine L from the known capacity and pitch for several pitches; and we then used that with readings from the condenser to determine the other unknown pitches. Probably what we called L was partly C in parts of the circuit other than the condenser, but that would not affect our comparisons.

For a more adequate idea of the apparatus itself, the accompanying diagram may help.

A supplies filament with current to heat it, thus providing an electronic discharge from the filament to the plate.

B provides an independent voltage from the plate to the filament; controllable by lamps.

C Grid circuit through middle inductance.

D Oscillating circuit composed of variable condenser and upper inductance, producing by its oscillations differences of potential between the grid and the filament.

E Telephone circuit in which are produced oscillations by virtue of the variable coupling of the upper and lower inductances.

If we consider the filament to be hot, thus supplying free electrons, and the 220 volt supply to be sending a current through circuit *C* from the plate to the filament and through the lower inductance back through the lamps to the plate, there will be thus induced in the upper inductance a voltage which will create an alternating current in circuit *D*. This circuit

will have a frequency dependent upon the natural frequency of the circuit D, which may be evaluated as approximately $f = \frac{1}{2\pi\sqrt{LC}}$, in which L is the value in henrys of the upper inductance, and C is the capacity in farads present in the variable condenser. The frequency, f, is measured in cycles per sec.

The alternate charge and discharge of the condenser will cause changing differences of potential between the grid and the filament of the vacuum tube, and these changes produce in turn relatively large changes in the electronic flow in the plate current, C. These changes in the latter circuit again induce magnified voltages in the upper inductance. The cycle repeats itself, providing larger and larger currents in circuit C, until it effects a maximum of variation of the current in circuit D, where the "static" condition of an alternating current constant in its effective value has been reached.

This "steady" alternating current in circuit D induces a similar voltage in the lower inductance, which in turn sends it through circuit E. E thus has the same frequency as D.

The above summary represents the plan of apparatus necessary to produce a pure tone of constant pitch in the telephone receiver.[2] With the exception of the receiver itself, the apparatus is arranged on a rubber-covered table in E's room. The receiver is isolated in the adjoining sound-proof room and is held constant in a standard. Further to insure the same position, the end containing the plate is inserted in a hole in the center of an upright lead sheet 14 by 14 in. This lead plate incidentally absorbs the sound waves which go back, and prevents reflection.

Method and Results

Briefly, the essential elements in the experiment are, first, to determine the amplitude of vibration of the telephone plate for some known current strength; secondly, to measure the current which produces a liminal intensity; and finally, to measure the energy in ergs which effects the liminal sensation in the ear for various pitches.

The physicist's definition makes of intensity that quantity of energy which passes in unit time through unit area of a surface placed at right angles to the direction of propagation of the sound. It depends primarily upon three factors: the amplitude and rate of vibration of the vibrating medium, the distance between the ear and the vibrating source, and the area of the vibrating source. The first two factors comprise the variables in the Wien-Rayleigh formula, which we use to establish the relation between strength of current and the intensity of the resulting tone.

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[2]The telephone used is one furnished by Kohl as part of Wien's instrument for measuring amplitude of vibration of the plate by means of a light wave reflected from a mirror on a lever. In the center of the plate is fastened a small steel rod to which we attached a glass rod for our linear calibrations.

$$A = \frac{C\, p_0}{2k} \cdot \Delta^2$$

$$\Delta = 0.147 \frac{k}{C^2} \cdot \frac{(2N)^2\, A\, R^2}{q}$$

A = energy per sec. passing through a square cm. of surface
C = rate of transmission of sound
p = own tone of the vibrating plate
k = index of specific heat
N = rate of tone (pitch)
R = radius of plate
Δ = relative pressure amplitude
q = distance between the ear and the plate

Before beginning the actual experiment, it was necessary to calibrate the different units of the apparatus to absolute terms. The amplitude of vibration for different current strengths was determined by attaching a very fine glass rod to the center of the plate with beeswax, and measuring its oscillations through a micrometer eye-piece. The current was read in terms of scale divisions on the string galvanometer with readings from an ordinary D'Arsonval and a rectifier of molybdenum used as a check for the lower tones.

Further calibration established the translation of the plate vibrations to galvanometer oscillations, and a consequent reduction of both to amplitude in mm. When these factors are known, it becomes relatively easy to determine the amplitude of vibration effected by the minimal intensities of various pitches; and, supplying this variable to the Wien formula, the total energy in ergs may be computed. A considerable amount of preliminary practice work was done with one standard tone of 120 vd., for which students from the elementary psychology classes acted as *O*s. While disparity in individual sensitivity was markedly apparent, an average of these measurements established the liminal energy for this tone in the region of 5.10^{-9} erg.

With the consequent addition of the tuning apparatus, more factors have become involved in the computation, but the method has remained essentially the same. The experiment lasts about one hour, with frequent rest intervals to prevent fatigue. The *O* is seated in the sound-proof room, the distance between the ear and the telephone plate being kept a selective constant by means of a head and mouth rest (sealing-wax biting board.) Reactions are transmitted to the apparatus room by means of a simple code of four responses made with a telegraph key and sounder, and representing the appearance, disappearance, decrease, and increase in the stimulus intensity. Verbal communication, though rarely necessary during the trials, is available by means of a rubber tube inserted through the wall.

The work of *E* involves the establishing of a desired pitch by combining certain units of capacity, and the adjusting of the inductance coils to provide a standard amplitude of current through the string galvanometer. The method of minimal changes has been used for the majority of the measurements, the limen being established, first, by subtracting units of resistance until the tone is heard; secondly, by adding them until it declines to inaudibility. In each step, some specific intensity is used as a constant to add to or to substract from. The determination of Weber's law follows the same general method.

With the high tones above 4,000, the amplitude of vibration becomes difficult to measure accurately, even in so delicate an instrument as the Edelmann galvanometer, and the readings are consequently recorded in

terms of unit resistance. For this purpose we have found the Leeds-Northrup rotating model to be of valuable assistance to ease and accuracy. In this series the current is left constant, and the telephone shunted around a resistance, the current through it being decreased by changing the resistance.

The bulk of the experiment was performed with 6 trained *O*s, all of whom were advanced students of psychology or assistants, including Miss Mary Palmer and Miss Sugi Mibai, graduate students, Mr. Richard Page, Mr. Adelbert Ford, Miss Edna Gordon, and Mr. Ernest Skaggs, assistants. Professor Pillsbury occasionally acted as observer, and was an indefatigable source of assistance in every phase and problem of the work.

The results of the work on these comparative limens are combined in Table B.

Prior to the addition of the tuning apparatus for different pitches, some preliminary work was done on Weber's law with a constant stimulus of 120 vd., the tone induced through the plate by a 60-cycle alternating current. The first method utilized a sliding rheostat for grading the intensity. Following the establishment of the individual limen, the tone intensity was increased or decreased by very slow manipulation of the rheostat; and whenever *O* reported an audible difference the current intensity was read in scale divisions on the string galvanometer and recorded. The averaged results from the 6 *O*s produce a general fraction of .2844, with a P.E. of .069.

In the second method, the telephone was placed in shunt with a Leeds-Northrup resistance box, and the tonal intensity was varied by passing the greater part of the current through the resistance. The readings in this series were recorded directly from the variations employed by *E*, and hence the table values read in ohms. Selecting first a minimal and than a maximal point, the limen was first established as in the preceding method. Resistance was then increased or decreased by steps of 1, 2 or 5, etc., the *O* reacting whenever he noticed a difference in the intensity of the tone in either direction. In this method, the shunt box was off the circuit, and the ear placed close to the telephone. The resulting fraction of .3152 is somewhat larger than that obtained by the first method, while its P.E. of .9422 is a trifle less.

In order to see whether different pitches would exhibit the same appreciable gradations of intensity, the establishing of certain limens was followed, where time permitted, by further experiments on Weber's law. The tones were selected from different portions of the scale, including particularly the areas surrounding the apparent turning points of intensity. In each instance, the limen as determined by previous experiment for a specific tone was used as the initial intensity or starting point.

TABLE B

Tone	Pr	Pg	M	Go	Fo	Sk	Wien
*d*120	5.10^{-9}	8.10^{-8}	2.10^{-7}	8.10^{-11}	1.10^{-9}	1.10^{-9}	
*d*341⅓	6.10^{-13}	3.10^{-11}	2.10^{-10}				
d 384	4.10^{-13}	9.10^{-12}	5.10^{-10}				
400							3.10^{-14}
d 426⅔	7.10^{-12}	6.10^{-11}					
c 512	5.10^{-13}	3.10^{-11}	3.10^{-10}				
d 576	3.10^{-12}	3.10^{-11}					
d 640	3.10^{-12}	2.10^{-11}	4.10^{-10}				
800							7.10^{-15}
c 960	3.10^{-13}	2.10^{-12}					
*d*1189	2.10^{-11}	9.10^{-11}	8.10^{-10}				
1600							1.10^{-15}
*d*1726⅔	1.10^{-13}	5.10^{-12}					
*c*2048	1.10^{-13}	4.10^{-13}	1.10^{-12}				
*c*2304	2.10^{-13}	4.10^{-13}	3.10^{-11}				
*c*2560	6.10^{-14}	6.10^{-14}					
*c*3072	2.10^{-14}	2.10^{-14}	5.10^{-12}				
*c*3200							5.10^{-16}
*c*3413¼	1.10^{-15}	5.10^{-14}	3.10^{-12}				
*c*3840	8.10^{-15}	3.10^{-13}	7.10^{-13}				
*c*4096	4.10^{-15}	7.10^{-14}	1.10^{-12}				
*c*5120	2.10^{-14}	6.10^{-13}	1.10^{-11}				
*c*6400	1.10^{-14}	8.10^{-14}	3.10^{-13}				3.10^{-15}
*c*8192	3.10^{-13}	5.10^{-13}	4.10^{-11}				
*c*9216	3.10^{-13}	6.10^{-13}					
*c*10240	6.10^{-12}	3.10^{-12}	9.10^{-11}				
*c*12288	9.10^{-11}	2.10^{-12}	3.10^{-11}				5.10^{-14}
*c*13650⅔	7.10^{-11}	8.10^{-9}					

With this as a basis, the *O* reported the first audible increase between this standard tone and a series of comparison tones of different intensity. This intensity then in turn became the starting point for a further increase, and so on up the scale. In the descending order, *E* gave the tone of maximal intensity as the starting stimulus, and *O* reported the first noticeable decrease in intensity, each subsequent determination becoming, in turn, a comparison basis for an ensuing weaker intensity.

A practical example may afford a clearer explanation of the specific measurements here. For instance, the liminal intensity for a tone of 512 vd. read, in terms of resistance, as 10 ohms. With this used as the initial intensity, *E* then decreased the resistance until *O* responded to a noticeable increase in the tone. Five such trials were made around this liminal value, and their average was accepted as the boundary of the first "step." The resistance was then set at this average, and this intensity was given as a standard or starting tone for the second step. The intensity was again increased until *O* designated his awareness of a change, the five repeated trials occurring here also as checks; and their average was accepted as the upper measure of the second step, or the lower standard intensity for determining the third increment. The procedure for the descending series of intensities was simply a reversal of direction, using this same method.

Table C includes the results on Weber's Law as hitherto determined. The fractional increment in this case represents an average for 14 ascending and 14 descending steps.

Translated into terms of intensity, the first step corresponds to a liminal sensation, while the maximal or fourteenth step provides a tone very easily audible throughout the room. The quantitative equivalents in ergs for these maximal points have not yet been computed, but it may be said in general that the fourteenth step is comparable in intensity to the average conversational tone of the speaking voice. Accepting this general hypothesis, and the apparent validity of Fechner's law, it seems safe to assume that the complete range of intensity gradations for the human ear would not exceed 100.

In these experiments, we have put into actual practice Fechner's suggestion for the measurement of sensations by using the just noticeable difference as the unit of comparison. Audition furnishes the ideal sense for these measurements because of the large fraction of Weber's law, and hence the small number of steps required. Our results indicate that there are but fourteen different steps, *i.e.*, fourteen different units, between the limen and a tone as loud as the ordinary speaking voice.

The acquisition of results from the louder regions of intensity has been somewhat curtailed by this method, owing to the 1000-ohm limit of the resistance box. It is highly desirable to extend the number of steps to as loud an intensity as can be practically obtained, and possibly this phase of the work will be carried on later in our laboratory. The results so far obtained, however, indicate that the principle of Weber's law does

TABLE C

Rate	Palmer Limen	Palmer Fraction	Palmer P.E.	Page Limen	Page Fraction	Page P.E.	Mibai Limen	Mibai Fraction	Mibai P.E.
120	5.10^{-9}	.4152	.0422	8.10^{-8}	.3961	.031	2.10^{-7}	.433	.0106
384	4.10^{-13}	.2792	.0847	9.10^{-12}	.2982	.074	5.10^{-10}	.4012	.125
512	5.10^{-13}	.26	.0276	3.10^{-11}	.304	.0313	3.10^{-10}	.418	.083
960	3.10^{-12}	.3031	.008	2.10^{-12}	.2054	.73			
1706⅔	1.10^{-13}	.209	.0658	5.10^{-12}	.293	.1037			
2048	2.10^{-13}	.196	.0712	4.10^{-12}	.2902	.081	1.10^{-12}	.3011	.086
2560	6.10^{-14}	.143	.106	9.10^{-13}	.286	.008			
3413½	7.10^{-15}	.273	.0296	5.10^{-14}	.1832	.0915	3.10^{-12}	.215	.0416
3840	8.10^{-15}	.2021	.046	3.10^{-13}	.2650	.993	7.10^{-13}	.2974	.0642
4096	4.10^{-15}	.1779	.0531	7.10^{-14}	.201	.7261	1.10^{-12}	.268	.072
6400	1.10^{-14}	.1462	.114	8.10^{-14}	.205	.05	3.10^{-13}	.2739	.084
9216	3.10^{-13}	.304	.091	6.10^{-13}	.495	.137			
12288	9.10^{-13}	.528	.1731	2.10^{-12}	.5634	.1009	3.10^{-11}	.6743	.017
13650⅔	7.10^{-11}	.4794	.092	8.10^{-9}	.8374	.209			

hold rather consistently for sound within certain limits, although the fraction is in no two instances identical, probably because of the errors of observation. The fraction near the limen shows consistently the lower deviation from the Law. With certain pitches, particularly in the lower range, it approaches one-half; in the medium range, it seems to maintain itself roughly at one-third or slightly less; in the upper range of the tones we used, around 12288 vd., it again approaches one-half or more.

In contrast to the conclusions of Smith and Bartlett, our data establish a slightly higher limen, in many cases, for the descending order of intensities than for the ascending, and the number of steps, when variable, is usually larger. No great divergence from these general tendencies was apparent in the results from different *O*s, the real contrasts occurring in the differential limens themselves and in different sensitivity to different pitches (Table B).

TABLE D

Table D includes fourteen steps from the ascending series of intensities for Miss Palmer. It serves to illustrate the variations in the fractional increments near and above the limen for tones selected from representative parts of the pitch range.

Steps	120 vd.	512	1706	2048	3840	6400	9216	12288
1	.468	.4325	.501	.4572	.571	.4218	.4273	.732
2	.4021	.3063	.4532	.396	.4032	.436	.561	.741
3	.4936	.491	.28	.4008	.401	.2997	.4821	.7064
4	.476	.3056	.278	.35	.312	.436	.4732	.623
5	.3495	.234	.2043	.2432	.2934	.171	.324	.544
6	.427	.2578	.3161	.2791	.2016	.203	.3041	.6921
7	.4241	.146	.2009	.268	.197	.1742	.3721	.5177
8	.305	.2932	.1937	.1104	.24	.2621	.209	.532
9	.3942	.205	.216	.3	.1911	.1287	.2573	.409
10	.401	.2005	.1867	.1873	.206	.1009	.3021	.5083
11	.43	.1893	.26	.29	.2173	.162	.256	.48
12	.372	.256	.1999	.19	.187	.2907	.305	.5112
13	.3589	.227	.254	.283	.272	.29	.49	.573
14	.47	.2436	.2126	.251	.33	.1894	.3783	.5805

Principal among the introspections from various *O*s was the observation that a maximal degree of attention during each experiment is necessary for valid judgments. The entrance of a fatigue factor always correlated with higher and more variable limens, and with enlarged and more uncertain increments of change. Careful consideration was, therefore, given to the *O*'s introspection of general feeling.

Some *O*s frequently reported auditory after-images and a difficulty in distinguishing the true minimal stimulus from subjective effects. This phenomenon apparently increased, rather than decreased, with practice, the higher sounds producing a more striking effect than the lower ones. In addition it may be

stated that the three *O*s having the lowest limens, and correspondingly the greatest acuity, encountered this difficulty most frequently. Two check-experiments with minimal values in intensity for 512 and 8192 vd., which employed the method of right and wrong cases, produced for them respectively an average of 78 and 81 correct perceptions, while for the *O*s less affected by auditory after-effects the general average of correct responses was 89%. The specific method in these two experiments was simply that of giving the warning signal 50% of the time when no current was on, and 50 % of the time when it was effecting a minimal intensity of vibration on the plate, and recording the percentage of correct and incorrect responses.

Kinaesthetic imagery seemed in general to predominate over visual, its effects occurring in a carrying-over of strain sensations of attention, and a feeling of effort to classify the different tones in some way.

The first three or four experiments for each *O* showed rather marked practice effects, but these did not appear subsequently in spite of repeated checks. The training seemed really to consist of increased attention and familiarity with reaction signals, rather than to involve any fundamental improvement in acuity.

One *O* reported difficulty in responding when the soundproof room was illuminated, but it is probable that darkness merely mitigated a possible visual distraction for him.

The three lowest limens were secured from musically-trained *O*s; the higher limen and the larger fraction belong to an Oriental student with no musical experience; but the real significance of these factors cannot be definitely ascertained without further experimentation.

Conclusions

That tones of different pitch correlate with different sensitivity in the human ear is indicated by the difference in energy required to elicit response to their liminal stimuli.

Tones of the upper middle range are more easily perceived than tones either above or below it. Our results place these turning points respectively in the regions of 1000 and 6400 vd. Within these regions are apparent inconsistencies which illustrate differences in sensitivity both for certain tones and for the individual ear which reacts to the stimuli.

For Wien, the point of greatest sensitivity lies in the region of 3200 vd. Our results approximate 3483⅓ for two *O*s, and 3840 vd. for the third *O*.

Weber's law as applied to audition apparently holds true with a fraction of about one-third throughout the middle range of intensities. The fraction is larger for low tones and for very

high tones. The fraction is also larger near the limen, decreasing universally in the third, fourth, or fifth step. Whether this result is consistent through the upper range of intensities it has not been possible to determine until our apparatus is modified to produce greater intensities.

Kinaesthetic and auditory imagery are evidently predominant over visual imagery for all *Os* in the experiment, and there appears to be some correlation between auditory imagery and the limen of tonal acuity.

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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDWIN G. BORING

XXV. THE EFFECT OF CHANGE OF INTENSITY UPON THE UPPER LIMIT OF HEARING

By E. F. MÖLLER

It has been suggested that the dependence of the upper tonal limen upon the intensity of the stimulus resembles the relationship that holds for the retinal color zones, where an increase in intensity of stimulus results in an extension of the zone such that a color of sufficient intensity may be recognized even at the periphery.[1] Savart[2] first observed that the upper tonal limen was different for different intensities of stimulus. Zwaardemaker[3] in 1893, and Scripture and Smith[4] in 1894, noted with the Galton whistle the general dependence of the limen upon intensity.[5] The latter concluded that "the general result for all observers indicates that the pitch of the highest audible tone varies directly and almost proportionately with the intensity." The Galton whistle has been subject to errors of calibration due to a failure to control the air-pressure during use and calibration,[6] but, although the absolute values of the limens of Scripture and Smith may therefore be doubted, the increase of the limen with increased air-pressure must probably be accepted, since changes in pitch of the whistle with overblowing would produce the opposite effect.

The present study is based upon experiments with steel acoustic cylinders of the Koenig type, actuated at various intensities by falling steel balls of different weights. Since the frequencies of the cylinders were calculated only, and not calibrated, the limens lack exact absolute meaning. The value of the study lies in the relationships of the relative frequencies as indicated by the psychometric functions obtained.

The stimuli employed were seven steel cylinders, selected from an octave, g^6—g^7, divided into twenty-one parts. This division did not give exactly equal musical intervals, since it was arranged to give all the diatonic intervals with the diatonic semi-tone (112 cents) divided into two, the minor tone (182 cents) into three, and the major tone (204 cents) into four parts. The scale thus consists of musical intervals of 56, 61, and 51 cents. The frequencies of the seven cylinders used (total range 341 cents; less than two whole tones) are shown in Table II.

It should be noted that these frequencies have been calculated by the manufacturers of the stimuli (Standard Scientific Co., New York), from a calibrated bar of greater length, on the theoretical assumption that the frequency varies inversely with the square root of the length. It would be desirable to calibrate the cylinders individually, but calibration is

[1] *Cf.* E. B. Titchener, *Experimental Psychology*, 1905, II, ii, 40.
[2] F. Savart, *Ann. chim. phys.*, 44, 1830, 340.
[3] H. Zwaardemaker, *Zts. f. Ohrenheilk.*, 24, 1893, 304.
[4] E. W. Scripture and H. F. Smith, *Yale Studies*, ii, 1894, 111.
[5] In general, *cf.* Titchener, *op. cit.*, 40.
[6] *Cf.* Titchener, *op. cit.*, 32-36. Professor Ruckmick of Wellesley College will shortly publish a paper dealing with the calibration of the Galton whistle and certain related artifacts.

difficult. Koenig[7] noted that the calculated frequencies fall short of the calibration; the manufacturers of our cylinders claim to have obtained better results. For the present purpose, however, a knowledge of the exact frequencies does not matter except for the determination of the absolute position of the limen. The psychometric functions which represent relationships of sensory response to the stimulus-continuum are essentially the same so long as the stimuli represent a continuum and do not involve artificial inversions. There is no reason to believe that a series of cylinders, varying from one another only in length, give anything but a series of frequencies increasing regularly with a decrease of length. And indeed, in an experience with these cylinders in the Clark Laboratory which extends considerably beyond the limits of this experiment,[8] the fact that no inversions in qualitative psychometric functions have ever been obtained, excepting only for *B* in this experiment, indicated further indirectly that the cylinders constitute a continuous series of frequencies.[9]

The cylinders were suspended in a semicircular trough of 15 in. inside radius, of 20 in. outside radius, and 3 in. deep. The sides of the trough were padded with felt, the bottom with cotton covered by felt. The cylinders hung in a radial position, suspended by loops of dental floss each from a pair of metal strips that projected over the two edges of the trough. There was a space of 1 in. between the projecting ends of every pair of supporting strips, which left room for actuating the cylinders by a falling ball. Adjacent cylinders were 10.6° apart, *i. e.* about 4.5 in. on the average between the axes or about 3.75 in. on the average between the sides of the cylinders.

Vibration was secured by dropping a steel ball-bearing upon the cylinder from an electromagnet attached to a rotating radial arm. The magnet was centered over the center-line of the trough: thus the arm could be swung so as to allow the ball, when released from the magnet, to strike any desired cylinder. A small piece of rubber tubing at the end of the core of the magnet just kept the ball from actual contact with the steel of the core. Attached to the magnet was a small pointer, which indicated the position of the magnet on a scale fixed on each metal strip, thus controlling the exact point of impact of the ball with the cylinder.

The ball was required to strike a glancing blow upon the cylinder for the reason that it was likely to bounce and strike twice when the line of fall was the vertical diameter of the cylinder. The point of impact selected was the point of emergence of a diameter of the cylinder that makes an angle of about 4°52′ with the vertical diameter. Different intensities of stimulus were obtained by using balls of different weight as indicated in Table I. These balls were, of course, also of different size, and thus gave different heights of fall from the magnet, which remained at a fixed height. The necessary corrections were small, however, in comparison with the difference of weight, and it makes little difference whether these results are computed in terms of the weight or in terms of the energy (weight with variable height of fall taken into account).

In order to render stimuli of different sizes comparable, the arm had to be adjusted so that the point of impact, *i. e.*, the point of tangency be-

[7] R. Koenig, *Wied. Ann.*, 69, 1899, 723.

[8] C. C. Pratt, this JOURNAL, 31, 1920, 403-406; and in another unpublished experiment by the present writer.

[9] It is a question whether the stimuli in the method of constant stimuli need to be equally spaced, or indeed whether such a statement of stimulus-distances has any psychological meaning. The matter is wrapped up with the problem of mental units and of the logic of mental measurement in general and can not be gone into here. This paper gives some indication, however, of the manner in which relative results may have scientific meaning without regard to their absolute values.

tween the spherical surface of the ball and the surface of the cylinder, should remain the same. The lateral displacement of the line of fall for a ball of radius r_1 with respect to the line of fall for a ball of radius r_2 is given by the formula:

relative displacement $= \sin a\ (r_1 - r_2)$,

where $a = 4°52'$, the angle of the diameter of impact with the vertical diameter of the cylinder. The displacements of the last column of Table I are determined in this manner.

TABLE I.

Dimensions of the stimulus. Radius, weight, height of fall, and resultant energy of the 6 steel balls, dropped by electromagnetic release on the acoustic cylinders for different intensities of stimulus. The last column gives the lateral displacement of the line of fall from the perpendicular diameter of the cylinder necessary in order to secure impact at the same point on the cylinder (*i. e.* 4° 52′ from the vertical diameter).

Ball No.	Radius (cm.)	Weight (gm.)	Height of Fall (cm.)	Energy (g.cm.)	Displacement from Center of Cylinder (cm.)
1	.225	.435	7.999	3.479	.67
2	.300	1.040	7.849	8.163	.68
3	.375	2.025	7.699	15.590	.69
4	.475	3.505	7.500	26.287	.70
5	.550	5.546	7.350	40.763	.71
6	.625	8.315	7.201	59.876	.72

The relationship for the heights of fall, h_1 and h_2, of two balls of radii r_1 and r_2 is:

$$h_1 = h_2 - (r_1 - r_2)\ (1 + \cos a).$$

The heights of fall in Table I are figured in this manner.

The total energy of the ball at the moment of impact is the product of the height of fall by the weight of the ball, but the energy effective for actuating the cylinder is that component normal to the surface of the cylinder at the point of impact. Since, however, the one is proportional to the other, it is enough to give the total energy, which is shown in the figures of Table I.

The observers were: Dr. C. C. Pratt (*P*), Mr. M. K. Macdonald (*M*), and Mr. F. L. Bixby (*B*), all highly practised in these judgments, since they had just completed observations on a similar problem, in which the same apparatus had been used.

The *O* was seated with his back to the apparatus and 4 ft. away. The instructions were read to him: "When the stimulus is presented, you are to say 'Yes', if you hear a tone, and 'No', if you hear no tone. Try to report immediately. Be sure of your judgment. If you are doubtful, ask to have the stimulus repeated. It is no matter how often you ask; never make a judgment when in doubt."

A few trials served to indicate a critical stimulus, which was then made the central stimulus in a series of five that included two cylinders on each side of the critical one. They were presented in haphazard order, with a 5-min. rest-period after each series of 100 judgments. A short preliminary warming-up practice preceded each series. One hundred judgments were taken on each cylinder for each intensity.

Table II shows the relative frequencies for the various calculated vibration rates and for the various intensities as indicated by the total energy of the falling ball. The rows of the table give the qualitative psychometric functions, which are plotted in Figg. 1-3, and the columns give intensive psychometric functions, which are plotted in Figg. 4-6.

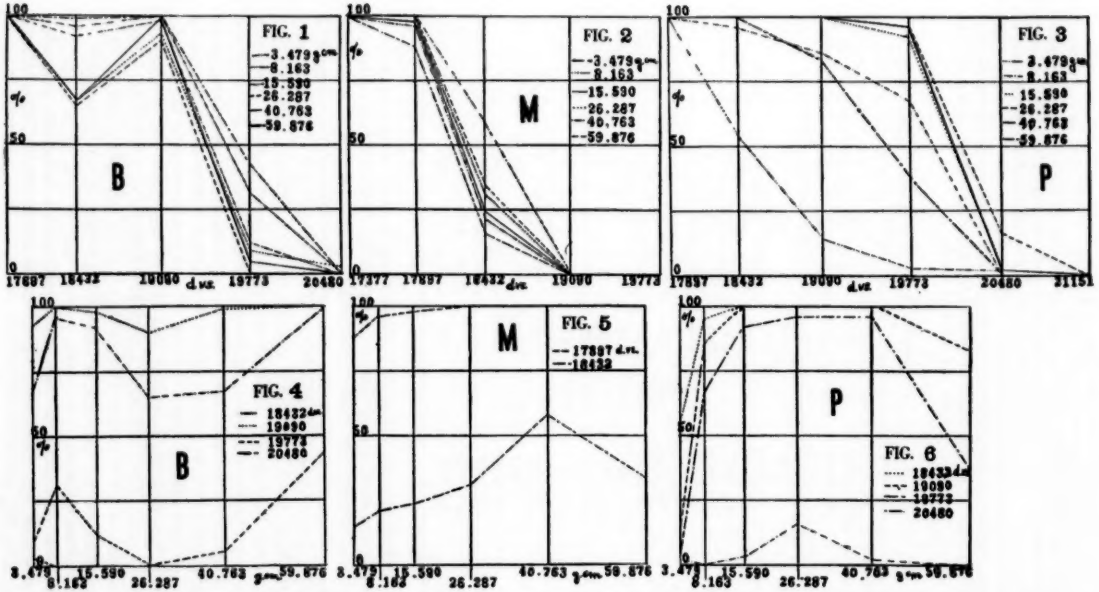
TABLE II

Observed relative frequencies of tones for various pitches (calculated vibration rates of the acoustic cylinders) and various intensities (energy of falling ball). The rows of the table give qualitative psychometric functions for the different intensities; the columns give intensive psychometric functions for different pitches. The 6 musical intervalls between the 7 stimuli in the table are successively from left to right: 51, 51, 61, 61, 61, and 56 cents.

Observer	Energy of Stimulus (g.cm.)	Calculated Vibration Rates of Stimuli (d.vs.)						
		17377	17897	18432	19090	19773	20480	21151
B	3.479		100	67	93	9	3	
	8.163		100	96	100	31	0	
	15.590		100	92	98	12	0	
	26.287		100	65	90	0	0	
	40.763		100	67	99	5	0	
	59.876		100	99	100	43	0	
M	3.479	100	88	15	0	0		
	8.163	100	96	21	0	0		
	15.590	100	98	24	0	0		
	26.287	100	100	31	0	0		
	40.763	100	100	58	0	0		
	59.876	100	100	34	0	0		
P	3.479		100	55	14	3	2	
	8.163		100	96	86	67	0	
	15.590		100	100	100	92	3	
	26.287			100	100	96	16	0
	40.763			100	100	96	2	0
	59.876		100	100	83	38	0	

In plotting the psychometric functions and in computing limens from them, we have used simply linear interpolation between the successive points. It is immediately evident from an inspection of the form of these psychometric functions that the *phi-gamma* hypothesis has no general validity for all psychometric functions: it is certainly not applicable here. We might have used Lagrange's formula as an indifferent hypothesis: a smooth curve that passes through all the observed points. A smooth function is probably more natural than a broken line, but, as Urban has shown, Lagrange's formula may lead to impossible interpolations, since it may give values above 100% and below 0%, and thus probably is equally in error in other regions. There is no particular justification for the straight line, except the practical one that it is easy to determine and renders interpolation easy. The work with Lagrange's formula is exceedingly laborious and there is no reason to believe that it gives any 'truer' result. It is good common sense, when we are equally ignorant of all hypotheses, to accept the least irksome.

By linear interpolation, then, we computed the terminal qualitative limens as a function of intensity. The results are shown in Table III and are charted in Fig. 7.



FIGG. 1-3. Qualitative (pitch) psychometric functions of tones for various intensities (energy of falling ball, g. cm.). FIGG. 4-6. Intensive psychometric functions of tones for various pitches (d. vs.). Observers *B*, *M*, and *P*. Graphic representation of the data of Table II.

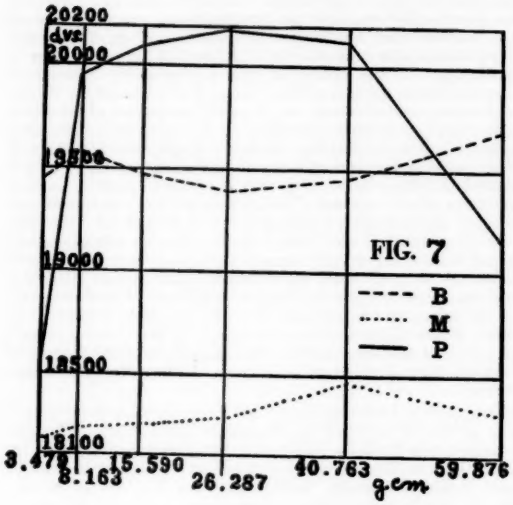


FIG. 7. Qualitative limens for various intensities of stimulus. Observers: *B*, *M*, and *P*. Data from Table III.

TABLE III

Qualitative limens, calculated from the rows of Table II by linear interpolation between the relative frequencies that include 50%.

Energy of Stimulus (g.cm.)	Observer		
	B	*M*	*P*
3.479	19,439.63	18,175.49	18,512.24
8.163	19,587.82	18,238.46	19,952.38
15.590	19,471.20	18,244.02	20,106.24
26.287	19,393.55	18,284.63	20,179.52
40.763	19,446.03	18,454.73	20,118.97
59.876	19,639.12	18,302.30	19,140.86

There are decided individual differences of qualitative terminal limen (Fig. 7). Except for the two extreme values of *P*, the individual differences are so much greater than the intensive variation for a single *O*, that the curves do not overlap.

The function that the qualitative limen is of intensity is different for different individuals. With increasing energy of stimulus, there is for *M* a decided rise followed by a decrease; for *B*, an increase, followed by a decrease, followed by a marked increase; for *P*, a great increase, followed by a great decrease (see Fig. 7). The form of the function is similar for *M* and *P*, but different in degree. *B*'s function is different from *M*'s and *P*'s, but similar in degree of variability to *M*'s. In no case are we able to say that the qualitative limen "varies directly and almost proportionately with the intensity."

A casual inspection of Fig. 7 might seem to indicate that the variation of the qualitative limen with intensity was insignificant and a matter of chance, perhaps of uncontrolled conditions. Inspection of Figg. 1-3 shows, however, that the relationships indicated by the form of curves in Figg. 7 hold consistently throughout the course of the psychometric functions. Fig. 7 is plotted for the limen defined as that value of stimulus most likely to give 50% positive judgments of tone. If, instead, the values of stimulus most likely to give other percentages are taken (*e. g.*, 75%, 25%), we find the same relationships holding. This fact is shown graphically in Figg. 1-3 by the fact that the various psychometric functions cross but rarely, that they have in general the same form, and that they lie in general in the same order throughout their courses. The argument for significant individual differences in these liminal functions is thus as follows: the differences cannot be an artifact of the cylinders, for the same relationship for a given *O* occurs with every stimulus capable of exhibiting difference; the difference cannot be an artifact of the different balls, for the same set of balls is involved for every cylinder for every *O*, and the different *O*s give different results; therefore the *O*s remain the only possible variant. It follows further that, if the differences in the course of the limen are significant, then the fact that the limen within these qualitative and intensive limits does not consistently increase with increase of intensity is also significantly established.

In general it is apparent that with material of this sort the mere statement of the limen gives but little of the available information. The interpreter of the data needs to keep the entire psychometric functions in mind if he is to have a complete knowledge of sensory response to stimulation at the upper limits of hearing. Especially does this fact appear in the case of *B*, Fig. 1. It will be seen that the relative frequencies for 18432 d. vs. are lower than for the stimuli on each side. One suspects at once a defect in calculation for the 18432 cylinder, but such an explanation will not hold. Neither *M* nor *P* shows an inversion at this point, nor did any inversions occur in extensive series taken in another experiment with these cylinders and with three other *O*s besides the *O*s of this experiment. Presumably therefore the group of psychometric functions may be taken as indicating for *B* a hypaesthetic region at 18432 d. vs. or else a hyperaesthetic region at 19090 d. vs. If the inversion in any of the psychometric functions had been great enough to cause the curve to cross the 50% abscissa, then it would have been possible to demonstrate statistically a tonal lacuna at 18432 d. vs. and a tonal island at 19090 d. vs., or, in psychophysical terms, a TR followed by an RL followed by a second TR. In a case of this sort it is apparent that no mere calculation of limens would ever give the total picture of auditory sensitivity. Even if we were willing to select some other frequency than 50% for the definition of the limen, we should not help ourselves, for there is no single abscissa that cuts more than three of the six psychometric functions, although all six functions demonstrate the same facts.

Figg. 4-6 show the intensive psychometric functions plotted from the columns of Table II. They represent the same facts taken from another aspect. Had the qualitative upper limen increased continuously with intensity, we should have been able to state the same fact by saying that the intensive lower limen (threshold) increased with pitch. In fact the psychometric functions of Table II are really not curves at all, but surfaces of relative frequencies plotted against pitch and intensity.

It is not possible in most cases to compute the intensive limens for the reason that a wide enough range of stimuli was not used. Two functions for *P* and one for *M* do cross the 50% abscissa. *P* would appear in this region to show a tendency toward an "intensive island." The function for 19773 d. vs. shows a lower limen at about 7 g. cm. and an upper limen

at about 56 g. cm. Between these two intensities, tone is heard more often than not. The other functions for *P* show the same general course, although they do not admit of the computation of limens.

There are individual differences among *Os* in the course of the intensive psychometric functions. *P* shows an increase followed by a decrease, both of an amount that is large in comparison with the qualitative change. *M* shows an increase followed by a decrease of amounts small with respect to the qualitative change. *B* shows consistently an increase, followed by a decrease, followed by an increase, all of a lesser degree than the amount of the qualitative change. It would thus appear that *B* has a region of intensive hypaesthesia followed by a region of intensive hyperaesthesia, that is to say, there are certain intensities which of themselves tend to increase the relative frequency with which tone is heard and which are more effective than are higher and lower intensities. The significance of these psychometric functions is indicated, as it was for quality, by the consistency among the functions for each *O*.

The degree of precision of the sensory response at these ranges of the tonal scale is indicated by the interquartile range of the qualitative psychometric functions, *i. e.* the change of pitch which would change the relative frequencies of the report of tone from 25% to 75%. In the eighteen qualitative psychometric functions, the interquartile range varies from 250 d. vs. to 830 d. vs., with an average of 494 d. vs. This average corresponds to a musical interval of about 40 cents, which is less than a quarter-tone, and shows a sensitiveness of discriminatory response not ordinarily expected in the region of the upper limit of hearing.

Conclusions

1. A complete account of sensitivity in the upper regions of hearing can not be given by the computation of limens; the complete psychometric functions must be considered.

2. Both qualitative and intensive psychometric functions can be determined simultaneously; the former indicates the qualitative upper limits of hearing, the latter the intensive lower limits for these qualities.

3. For any given *O* the qualitative psychometric functions for the different intensities are similar, and the intensive psychometric functions for the different pitches are similar.

4. There are individual differences in the forms of both the qualitative and the intensive psychometric functions. Both kinds of functions may show significant inversions or reversals and are not even approximately ogival in form.

5. Qualitative sensitivity, as indicated by the limen and also by the entire psychometric functions, does not, within the limits of this experiment and for these three *Os*, increase consistently with an increase in the energy of the stimulus, but follows a less simple law which varies for the individual *O*.

6. The qualitative psychometric functions indicate for one *O* the existence of a qualitative region that is hypaesthetic with respect to the next higher pitches. This phenomenon is presumably similar to the phenomenon of a tonal lacuna, but less extreme. Similar variations occur in the intensive psychometric functions.

7. The interquartile range of the qualitative psychometric functions is on the average about a quarter-tone, indicating an unexpected sensitiveness of discriminatory response.

XXVI. A Study of the Relation of Distracted Motor Performance to Performance in an Intelligence Test

By Miles A. Tinker

This investigation was undertaken with the hope of ascertaining whether there is any relation between motor performance under distraction to performance in an intelligence test. Often, at the end of an intelligence test, one hears the complaint that certain distractions, such as "nervousness" or the necessity for working against time, prevented the subject from doing his best. It was our intention to see whether these reports of "nervousness" or an objective test of distractibility would show a negative correlation with performance in the intelligence test.

Our results in the end were negative. No correlation was apparent, in part for the reason that the distractor instead of distracting proved a spur to attention. We are presenting the findings, nevertheless, because it is of value to have the outcome of such an attempt known, and because the results show in a striking manner the fact, already known in other contexts, that a distractor may prove an aid rather than a hindrance to many kinds of performance.

We had 39 subjects, of whom 33 were naive and 6 were members of the graduate department of experimental psychology.

All subjects were given the Otis Group Intelligence Scale; Advanced Examination, Form A.[1]

For the motor test for distracted performance we used two mazes, designed with a single univocal path without bifurcations in order to test speed and steadiness of motor performance but not learning. Each maze consisted of a path which wound from the periphery to the center of the maze and then out again according to the plan of the "walls of Troy." The total length of a maze is 72.5 in., and a maze covers a space of 11 by 9 in. The walls of the path were made of 0.25 by 0.25 in. brass strips, screwed to a wooden base covered with celluloid, with a path one-eighth in. wide left between. The two mazes were identical with the exception that one had notches one-sixteenth in. deep and one-eighth in. long, separated by one-eighth in., all along the walls of the path. The notches in one wall were directly opposite those in the opposing wall. A metal stylus, one-sixteenth in. in diameter, was used to trace the path. The stylus and the maze were connected electrically with markers on a kymograph so that a graphic record of all contacts of the stylus with the walls of the maze was obtained. A time-line on the record gave the time for traversing the maze. At *S*'s ear on top of a post was a cigar box, for a resonator, with an electric bell attached. The bell was wired so that, when a switch was closed, it would ring every time the stylus came in contact with the wall of the maze.

The notched maze was intended to place a greater emphasis upon accuracy as against speed, since *S* might catch the stylus in the notches if he hurried. As a matter of fact, however, the notching reduced accuracy as well as speed, so that the introduction of the notched maze did not enable us to reduce accuracy and speed to a single variable as we had hoped to do.

The ringing of the bell close to *S*'s head when a contact was made was intended to be a distraction and to induce "nervousness."

[1] *Jour. of Educ. Psychol.*, 1918, 9, 239-261; 333-348.

Procedure

The intelligence test was given according to the directions in the Manual[1] except for the following reductions in the time allowed for certain of the tests: test 4, from 6 to 4 min.; test 5, from 6 to 5 min.; test 7, from 3 to 2.5 min.; test 8, from 4 to 3 min.; test 9, from 6 to 4 min.; test 10, from 3 to 2 min. We made this change because we have found from experience that, if the full time is given, too many finish before the time is up.

At the end of the test the *S*s were asked to answer certain questions intended to reveal the degree of "nervousness" which they experienced in taking the test. The following directions were read to *S*:

"This is a test of steadiness. You are to take the stylus and try to trace the path through the maze without toucing the sides. The problem is to get through in the shortest possible time with the least number of touches. Every time you touch the side the error is recorded electrically and counts against your score."

The order of tracings in the two mazes was as follows:

1. Smooth } No distraction
2. Notched }
3. Notched }
4. Smooth }
5. Smooth — Unexpected distraction
6. Smooth }
7. Notched } Expected distraction
8. Notched }
9. Smooth }

Before the fifth tracing the switch which connected the bell was closed without the knowledge of *S* and the bell rang every time a contact was made. Before the sixth tracing *S* was warned that the bell was still attached and would ring every time that he touched the wall of the path with the stylus.

At the end of the tracings questions were again asked in order to bring out *S*'s opinion of his degree of "nervousness" in performing the test.

Results

Averages for the results of the motor performance test and of the intelligence test are given in Table 1, and the significance of some of the differences between these averages is indicated in Table 2.

In the first four rows of Table 1, if we compare the tracings in the smooth and notched mazes for no distraction and for the expected distraction, we find that both the time of tracing the maze and the number of contacts vary in the same direction and in approximately the same degree. For this reason we felt justified in combining the results for a distractor and for no distractor in the fifth and sixth rows of the table, in order to show the general difference between the smooth and notched mazes. Originally we had introduced the notched maze with the intention of rendering a rapid traverse of the maze difficult and thus of placing a premium upon accuracy as against speed. We had thought in this manner to obtain some comprehension of the supposedly inverse relationship between speed and accuracy, and thus to be able to reduce the two measures of performance to one. We find, however, that the relation is not inverse, for the introduction of the notches not only lengthens the time but also decreases accuracy (in spite of the fact that the notches reduce the probability of contact since the path is wider between opposing notches). It is apparent

[1] *Otis Group Intelligence Scale, Manual of Directions*, 1920.

therefore that we must in this case regard speed and accuracy in the maze as at least partially independent variables, since they vary together and not inversely when the change is made from the smooth to the notched maze.

It appears also from Table 1 that the effect of the introduction of the expected distractor is similar in direction and amount for both the smooth and the notched maze (see the first four rows). Accordingly we have combined these results in the seventh and eighth rows so as to show the general effect of the introduction of a distractor. It is here evident that the presence of the distractor decreases the speed and increases the accuracy. Such a result is equivocal, for the reason that we can not tell whether the decrease of speed is responsible, at least in part, for the increase in accuracy. We have seen that speed and accuracy may vary independently as they do when notches are introduced in the walls of the maze; it is natural therefore to regard them as independent here. The argument for their independence is, moreover, considerably strengthened by the results from the introduction of the "sudden distractor," *i. e.*, the distraction begun without warning to *S*. In the ninth row of Table 1 we see that speed was not appreciably changed by the distraction, whereas the accuracy was greatly increased. Certainly, then, the variation must have been independent in this case, and it seems highly probable that it is to be so considered when the expected distractor is compared with the normal case.

What happened must have been approximately as follows. In the first four trials the *S*s settled down to a given degree of speed and accuracy for each of the two types of mazes. In the fifth trial they were startled into a higher degree of attention by the unexpected ringing of the bell whenever an error was made. The average speed of the preceding trials was maintained approximately, but the "distractor" acted as a spur to attention and the *S*s consequently worked with much greater precision. In the subsequent trials where the distractor was continued they may have been fatigued: the *S*s slowed down and became less accurate than with the sudden distraction; they remained, however, more accurate than in the initial undistracted trials, either because of the attentive spur of the distraction or because of the slower rate. That intended distractors, especially when intermittent, may fail to distract and may instead act as a spur to attention and thus lead to intensification of impression or to shortened reaction time is well known.[2] In our experiment some of the *S*s commented upon the steadying effect of the ringing of the bell, and also upon the advantage that it gave them in notifying them when they made a contact. On the other hand many *S*s, who reported that the test had made them "nervous," stated that the bell made them especially nervous. It is not impossible that the bell not only made the *S*s "nervous," but also spurred them on to better work, and that in general the conditions of "nervousness" in such a test—and perhaps also in an intelligence test—may also be the conditions of accurate performance.

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[2]H. Münsterberg and N. Kazaki, The Intensifying Effect of Attention, *Psychol. Rev.*, 1894, 1, 39-44; A. J. Hamlin, Attention and Distraction, this JOURNAL, 1896, 8, 3-66; J. E. Evans, The Effect of Distraction on Reaction Time, *Arch. of Psychol.*, 1916, no. 37, vol. 5, 1-53; E. E. Cassell and K. M. Dallenbach, The Effect of Auditory Distraction upon the Sensory Reaction, this JOURNAL, 1918, 29, 129-143.

TABLE 1. Average performance in speed and accuracy in motor test of steadiness. Figures are averages for time (secs.) and number of contacts with the walls of the maze for 39 subjects. The numbers in the second column are series numbers. Performance on the Otis intelligence test is shown in the last two rows.

		Time		Contact	
		Av.	M. V.	Av.	M. V.
No distraction	Smooth maze: 1, 4	76.4	21.2	74.5	15.6
	Notched maze: 2, 3	86.0	20.2	109.8	24.6
Expected distraction	Smooth maze: 6, 9	82.5	30.3	61.6	15.2
	Notched maze: 7, 8	92.5	25.9	93.9	21.2
Smooth maze	1, 4, 6, 9	79.8	20.0	67.9	13.1
Notched maze	2, 3, 7, 8	91.6	21.4	100.4	21.1
No distraction	1, 2, 3, 4	81.1	18.5	90.3	15.5
Expected distraction	6, 7, 8, 9	90.5	27.2	79.8	14.0
Sudden distraction	5	82.4	33.2	69.2	16.5
Otis test	Score	155.5	26.2		
	% accur.	.859	.070		

Table 2. Significance of differences between averages of Table 1. The table shows the significance of the difference found in changing from the smooth to the notched maze, and in introducing a sudden or an expected distractor during the performance. D/P.E.D., the ratio of the difference to its probable error, is the usual measure of significance; P_D, the probability of difference, is, on the assumption of the normal law, the probability that the difference will not vary from itself by an amount more than itself.

		$\frac{D}{P.E._D}$	P_D
Smooth *vs.* notched maze	Time	2.98	.956
	Contacts	9.67	1.000
Sudden *vs.* expected distraction	Time	1.39	.651
	Contacts	3.63	.986
No distraction *vs.* expected distraction	Time	2.11	.845
	Contacts	3.73	.988

Table 3 gives the results which the problem was planned to educe. It was hoped that change in performance in the maze under the introduction of a distractor would prove an objective measure of distractibility

or "nervousness." A significantly high negative correlation of this change with performance in the Otis test might indicate that such distractibility as an individual characteristic was a special disadvantage in an intelligence test; whereas a high positive correlation might have meant that the distractor acted as an attentive spur and that "nervousness," so defined, was of advantage in taking an intelligence test. As a matter of fact, however, the correlations are all low. Since speed and accuracy seem to be independent variables in the maze, they had to be treated separately, and the effect of the one or the other eliminated by partial correlation. In the same manner score and accuracy on the Otis test were separately cared for. The last column of Table 3 shows, however, that distractibility neither as measured by speed (T) nor as measured by accuracy (C) is highly or significantly correlated with either of the measures of performance on the Otis test, even when correction is made by partial correlation for variation of the two factors not entering into the correlation.

TABLE 3. Coefficients of correlation (products-moments method) and partial coefficients of correlation between T, C, S, and A, defined as below. The probable errors of these coefficients are all between .06 and .10.

T = change in time for traversing maze when expected distractor is introduced.

C = change in number of contacts made in traversing maze when expected distractor is introduced.

S = score in the Otis intelligence test.

A = accuracy (ratio of items right to items attempted) in the Otis intelligence test.

$r_{TC} = -.37$	$r_{TC.S} = -.39$	$r_{TC.A} = -.39$	$r_{TC.SA} = -.39$
$r_{TS} = -.08$	$r_{TS.C} = -.16$	$r_{TS.A} = -.10$	$r_{TS.CA} = .07$
$r_{TA} = -.10$	$r_{TA.C} = -.18$	$r_{TA.S} = -.06$	$r_{TA.CS} = -.13$
$r_{CS} = -.17$	$r_{CS.T} = -.21$	$r_{CS.A} = -.08$	$r_{CS.TA} = -.13$
$r_{CA} = -.17$	$r_{CA.T} = -.21$	$r_{CA.S} = -.08$	$r_{CA.TS} = -.13$
$r_{SA} = .67$	$r_{SA.T} = .67$	$r_{SA.C} = .67$	$r_{SA.TC} = .67$

There is little to be gained from the results of the questionaries given the *Ss* after the intelligence test and after the maze trials. The *Ss* tended to follow the suggestion of the questions and to admit "nervousness." There were 21 who reported themselves "nervous" in both the intelligence and the maze tests; 6 who admitted "nervousness" in neither; and 12 who were "nervous" in one but not in the other.

Conclusion

At first sight our results seem mainly negative. The introduction of an auditory distraction during a test of motor steadiness may not distract the *S*, even though he reports a conscious disturbance, but may spur him on to more accurate manual performance. The distractor has a measurable effect, howbeit in an unanticipated direction. The sensibility of an individual to this sort of effect is not, however, prognostic of his performance in an intelligence test.

There is, however, an application of a known psychological fact which the experiment renders the service of indicating. We have known that

sensory impression may be reenforced attentionally by the facilitating effect of an intended distractor that does not distract, and that reaction times may be shortened in the same manner. We have now shown that manual precision of movement may similarly be increased by the concomitance of an intermittent intended distractor. Obviously the next experiment is to make the analogous direct attack upon the intelligence test itself. May it not be that the presence of a distractor will improve the performance of a group in an intelligence test? At any rate we know that complaints about distraction have in themselves little value as bearing upon the distracted performance. It may be unpleasant to be "distracted;" it is generally unpleasant to have the attention spurred; but, pleasant or unpleasant, the spur may result in improved performance for the individual and thus justify itself in spite of the contrary opinion of the *S*. Even if an apparent distraction does not improve intelligent performance, we have certainly no way of knowing whether it interferes. The direct experimental attack needs yet to be made.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXI. The Areal and Punctiform Integration of Warmth and Pressure

By Ida Bershansky

This study was undertaken in the hope of resolving a difference in the results of recent investigations of the integration of warmth and pressure. Malmud had found only a close fusion of the two qualities, described at times as a warm-pressure, and at times as a pressury-warmth.[1] Cobbey and Sullivan, on the other hand, had reported a perceptive integration which they called 'oiliness'.[2] There was, however, a difference in method of attack. Malmud began her experiments by arousing simultaneously punctiform warmth and pressure, and her *O*s were asked to report the course of the resulting experience; she assumed that, if a perception appeared, it would be indicated. Cobbey and Sullivan first employed areal stimulation with the intent of determining the 'compulsory conditions of the oily perception', and then, after this perception was known, undertook a punctiform stimulation. They suggest that Malmud's negative finding might be explained by the difference in procedure; and we have, therefore, repeated their experiment.

Our experiment, like its original, was divided into two parts. In the first, we immersed the first joint of a finger in some oil or in warm water of known temperature, and we asked our *O*s to report the resulting sensory experience, particularly as regards the first impression, and then as regards any changes that might take place in course; they were also instructed to name the perception. In preliminary experiments we employed two *O*s, Miss G. Adams (*A*), and Miss M. E. Smith (*S*), both of whom were experienced in psychological observation; later we added as a third *O* Assistant Professor Hoisington (*H*). The *O*s were blindfolded and their nostrils were stopped with cotton-wool before they entered the experimental room. They sat with the right arm placed on an arm-rest which extended over the edge of a table, and which contained a hole through which a finger was thrust; this served to keep the arm and finger in a fixed position. The stimuli used were kerosene oil, olive oil, water, and castor oil, which were presented in the order named. The water was warmed by means of an electric coil; at the beginning of an experiment the temperature was 32° C., and during an observaion it was gradually increased to 38° C. or 40° C. The average temperature of the oils and of the experimental room was 21° and 20° C. respectively.

The procedure in a single experiment was as follows: *E* gave the signal 'now'; and then, slowly and evenly, by means of a mechanical device, raised a small glass vessel filled with water or oil, as the case might be, until the first joint of the finger was immersed; and *O* immediately began his report. The stimulation was continued for 10 min. unless *O* reported fatigue or heat. In order to remove any clue to the nature of the stimulus-object after the period of stimulation the finger was dipped in gasoline; and at the close of the experimental period the finger was washed with soap and water before the blindfold and the cotton-wool were removed.

[1] R. S. Malmud, this Journal, xxxii, 1921, 571.

[2] L. W. Cobbey and A. H. Sullivan, this Journal, xxxiii, 1922, 121.

In these preliminary experiments *S* in 38 trials reported 12 perceptions, 3 when kerosene, 1 when water, 3 when olive oil, and 5 when castor oil was the stimulus; *A* in 40 trials reported only one perception of an object. Characterizations of these perceptions are:

S. Olive oil. 'Just like putting the finger in fine flour; it is an oily smooth, a sort of thin oil;' 'tiny indication of oiliness like cold cream on the hands when you have not wiped it all off.' *Castor oil.* 'It feels like warm thin butter;' 'there is a little coolness in the pressure, it may be an oily damp; now it is a little greasier than anything else, it is a sort of combination of cool wet and grease;' 'there is just a little pressure and cool, an oily damp like sticking finger into lard.' *Kerosene.* 'Feels like putting the finger in cold oil; it is not a watery wet, but a smooth wet. It is a cool and smooth pressure, an oily damp.'

Warm water. 'It is a nice smooth warm; a thinner feeling of pressure than the oily feeling; the other feels more oily, this feels thinner.'

A. Kerosene. 'Just as if I had been touched by warm jelly; I cannot say what it was because it wasn't a familiar perception.' For the rest this *O* employed such terms as 'close pressure', 'soft warm pressure', 'a pressure which sticks close around my finger', etc.

At this stage of the experiment we were puzzled by the fact that *A* reported no more perceptions, and by the further fact that *S* with a single exception failed to get the perception with warm water. A study of the reports led us to believe that the contrast between the temperature of the oils and that of the water was a disturbing factor; and, since fatigue and adaptation appeared in nearly every experiment, we decided also that the period of stimulation was too long. We therefore raised the temperature of the oils to 30° before stimulation, lowered the initial temperature of the water to the same point, and resolved in subsequent experiments to remove the stimulus as soon as *O* had reported a perception. With these changes in method, and with *H* as an additional *O*, we began another series.

In this second set of experiments *S* reported 10 perceptions in 24; *A*, 2 in 28; and *H*, 25 in 31 trials. Perceptions for all *O*s are as frequent with warm water as with any other stimulus, and also with the thinner as with the heavier oils. The characterizations of the perceptions of *S* were as before. We tried to force the perceptive attitude upon *A* by asking her every time to name the perception; but she was still unable to do this; she repeatedly reported the experince as wholly unfamiliar; and the most that she could say was, for example: "I feel something soft and warm and something heavy closing in around my finger; it felt soft, but I did not get the perception of any object;" "I get some kind of perception but it isn't anything that I know; it felt something like squeezing the finger with a rubber glove." The one other instance of a perception of object was: "It was just as if I were touched by a warm jelly; I cannot say what it was; it wasn't a familiar perception." Typical perceptions of *H* are as follows: "I cannot objectify the perception because there is nothing like it in my experience;" "I don't know what to call the perception, but it is a snug cozy something that has considerable density, and lies tightly around the finger like oil;" "it is like a semi-liquid or a heavy oil only I do not get the smoothness as when I rub my fingers together;' "it is like a dense liquid that clings closely to the finger. It might not be a liquid; I am using the term inferentially; I cannot conceive of anything as acting on my finger like this except a liquid;" "a clinging oily liquid that fluctuates in density as the warmth fluctuates in intensity;" "it is like thin butter;" "like warm molasses without stickiness, or a heavy oil that was thick."

Psychologically, our *O*s have described the experience as a fusion of warmth and pressure which may be characterized as a "warm snug pressure" or as a "warmth that sticks close to the finger." In this respect there

seems to be complete agreement with the *O*s of Cobbey and Sullivan. All these, however, named the perception 'oiliness;' whereas our *O*s were not satisfied either as individuals or as a group with any one term as a name for the perception. *S* and *H*, it is true, employed the word 'oiliness' more often than any other; and they named objects that were oily more frequently than any others; but they also named objects that are not oily, such as glue, mercury, jelly, rubber glove, and molasses. If, therefore, we accept the term 'oiliness,' it must be with some reservation. It is well known that the common perception of oiliness involves movement. In our experiment every effort was made to prevent movement of the finger during the period of stimulation; and *H* says, as we have seen, that the perception under these conditons is "like a semi-liquid or heavy oil, only I do not get the smoothness as when I rub my fingers together." Furthermore, at the close of the experiment he states: "The perception is one that we get from liquids, including oils; it is just as much like mercury or molasses without the stickiness as like oil."[3]

A by-product of the experiment which is of psychological interest is the course of the perception with change in degree of temperature. If the initial temperature of the water (30°) was below that of the skin, the perception was almost invariably of 'wetness.' Following this, as the temperature increased, the perception changed first to a soft, snug warm pressure which meant a thin (sometimes oily) liquid; and then, with still higher temperature, to a closer, tighter, warm pressure which carried the meaning of a thicker, denser, more viscous liquid. The higher limit in intensity was about 36°; beyond this, the warmth became dominant in intensity and clearness, and the fusion was broken up; the warmth was then felt as a 'radiant warmth with a background of pressure.' The effect of the increase in warmth was to increase the intensity, *i. e.*, the closeness, snugness of the pressure. It will be noted that, under the conditions of our experiment, pressure is felt either over the entire surface or over patches of the skin *below the surface of the liquid stimulus*. The ring of the pressure gradient is felt, if at all, only when the temperature of the liquid is approximately that of the skin;[4] the *warmth* is felt as warm-pressure, and increase of warmth as increase in the intensity of warmth and pressure.[5]

In the second part of the experiment we employed a punctiform stimulation of warm and pressure spots. For the pressure stimulus we obtained excellent results by Cobbey and Sullivan's method of raising a hair to the vertical position; better still, by bending the hair backward. For the

[3]In the hope of furnishing a perception that would serve as a slight contrast to that of our experiments, and that might, therefore, aid in characterization, we occasionally and without warning exchanged the usual stimulus for one of flour warmed to a temperature of 37°. *S* reported the object of perception as 'flour or some powder;' *A*, as a granular, resisting substance; and *H*, as a dense, semi-liquid substance with a density like mercury and a clingingness more like oil.

[4]There were times when *O* did not know when his finger entered the liquid (water or kerosene), and there were also times when he got no experience while his finger was in the liquid. A ring of heat is sometimes felt when the temperature of the water is above 38°.

[5]It is, of course, possible that the pressure in this case may have its origin internally, and be referred to the surface. Internal pressure is, however, frequently reported, and we should still have to explain why increase in warmth of stimulus carries with it increase in felt pressure.

warmth we were most successful with Dimmick's electric stimulator. Our procedure was as follows. We first isolated warm spots which had a hair within their area or very near them. Then, in an experiment, we first aroused warmth; and when it was reported, raised the hair. Under these conditions we obtained a perception in 7 of 11 trials with *H*, in 5 of 24 with *S*, and in 1 of 35 trials with *A*. The naming of the perception was found to be much more difficult than in the areal experiments. Typical reports are: (*H*) "Qualitatively, I do not see much difference between this semi-liquid and the one my finger was in;" "I get the warmth and pressure beaten up in perception, it was oily in quality;" "the experience is like a drop of dense liquid; it is not wet; I don't know what to call it; it is a little like dense mercury, and a little like a heavy oil;" (*S*) "It is oily rather than wet;" "when the warmth and pressure are nearly equal I get the perception of oiliness;" "it feels a little like warm butter on the end of a toothpick. The perception does not come easily; it is hard to name it." The one perception of *A* was: "It feels sticky, like grease."

Psychologically, the experience is nearly always a warmish pressure; it is a fusion in the sense that it is unitary and yet may at any time be analysed into the two qualities. In the integration the pressure, which must be steady and not too intensive, seems to spread a little and to lose its sharpness of definition.

Conclusions. Our results appear to explain the divergence noted at the outset of this Study. There can be no doubt that the warmth-pressure integration, if it suggests an object of perception at all, most often and most naturally carries the meaning of oiliness. If, then, the *O*s are set for a one-to-one correlation of experience and perceptive meaning (and this set may be induced without any corresponding instruction from *E*), they will give a regular report of 'oily' in the synthetic experiment. In so far we agree with Cobbey and Sullivan.

But oiliness is not, under ordinary circumstances, a sheerly cutaneous perception. If, then, the *O*s are not set for perceptive report, the word 'oiliness' need never occur to them; and if they are set for perception generically only, and not specifically, they may vary in their reports,—they may fail to discover an appropriate term, or they may interchange 'oil' with such other substances as 'butter' and 'glue' and 'molasses,' or they may settle easily upon the single word 'oily.' In this way we account for the results of Malmud and for those of our present observers.

It seems, therefore, more nearly true to say that the integration of pressure and warmth is a compulsory part-condition of the perception of oiliness than to regard it as the single adequate condition.

LXII. The Integration of Warmth and Pain

By Lucile Knight

This study forms a member of a series undertaken to discover what results psychologically when warm, cold, pressure, and pain spots are taken in pairs and stimulated simultaneously.[1] In this investigation we have worked with warm and pain spots.

[1]For references to the earlier investigations, see R. S. Malmud, this Journal, xxxii, 1921, 571. See also J. H. Alston, *ib.*, xxxi, 1920, 303; and L. W. Cobbey and A. H. Sullivan, *ib.*, xxxiii, 1922, 121.

Before beginning the experiment proper we practised the technique of localization and stimulation of pain spots, and acquainted our *Os* with the qualities of pain. All spots were localized within an area 2 cm. sq., on the volar side of the fore-arm: the skin was first softened with soap and water, and then explored with a sharpened horse-hair 2.5 cm. in length. This stimulus was found to be inadequate to the arousal of pain when the skin was etherized. After two weeks of preliminary work we localized warm and pressure spots within the same area, and selected for experimentation spots that responded with a warmth of good intensity, and that had closely neighboring pain spots. We took particular care that no pressure spot was near enough to be stimulated in any way during the course of an experiment. In a first series of experiments we employed two *Os*, Miss E. Powell (*P*), and Miss E. Davis (*D*), both of whom were specializing in psychology. For the arousal of warm spots we used Dimmick's electrical apparatus, which stimulates the spots by radiant heat. This was so mounted that it could be lowered to a position about 2 mm. from the surface of the skin, and then, when warmth had appeared, could gradually be raised and the warmth still maintained. Our procedure in a single experiment was to arouse warmth; then, when it was reported, to give the signal 'now;' and after 1.5 sec. to stimulate the pain spot (which had been softened with vaseline) with the sharpened hair. The instruction to the *O* was as follows: "You will be given a cutaneous stimulation on the forearm. Report the course of the resulting experience. You may give a running account if you like."

The results obtained by this method were not satisfactory; the heat from the apparatus frequently not only curled and therefore destroyed the hair, but also dried the skin so rapidly that pain spots failed to respond. Both *Os*, however, gave reports which indicated that the conditions of an integration were at times obtained. The two qualities seemed either to form a spatial pattern in which they ran their courses side by side, or to fuse in a new quality which was called 'hotness'. From a study of the reports we felt moderately sure of this fusion; but we could not be certain that the 'hotness' was not occasioned by the radiant heat of our apparatus. *E*, it is true, always raised the apparatus to what she thought was a safe distance; but she had no other guide than the reports of her *Os*. We determined, therefore, upon a test experiment that should more adequately be controlled, and that should be undertaken with more experienced *Os*.

In this second series of experiments we mounted the electrical apparatus on a universal standard and let *O* himself, by turning the screw which raised or lowered the stimulator, control the degree of warmth. We also placed the arm in a plaster cast, took the additional precaution of selecting warm spots that were not contiguous to cold spots, and aroused pain by touching the skin lightly with a needle. The *Os* were Professor Weld (*W*), Professor Hoisington (*H*), and Dr. Bishop (*B*). The author also observed in a few experiments in which Professor Weld served as *E*. *O* was asked to adjust the apparatus until he felt a continuous warmth, and then to say 'now;' the pain stimulus was then applied, and *O* straightway described the resulting experience.

Under these conditions 'burning heat' or 'hotness' was usually reported. In general, the loose, comfortable warmth contracts about a hard penetrating stingy pain; the warmth is replaced by a definitely painful sting (heat). We were able to compare the fusion thus obtained with an actual burn by lowering the apparatus until burn appeared, then raising it until only warmth remained, and then stimulating with the needle. At first all *Os* were unable to distinguish the two experiences, but after a few trials an extensive difference was made out. Typical reports of the *Os* are as follows.

H. "The latter [with the needle] is a little less extended; qualitatively, I don't see any difference;" "a sharp one this time; it was more

limited in extent but quite as penetrating and as burny;" "I called 'now' when the other sting was there, so I had the two together. The sting from the heat was weak, that of the needle more intense and went deeper; qualitatively I could not say which was which. In general, the difference is one in intensity; the one with the needle is usually less extended, although they may be similar in extent; the actual burn is more like several points, but they are not so lively."

W. "The needle-burn was much more pointed, sharper, not so sting-like this time, but smaller and more punctiform. Qualitatively, the two experiences are exactly alike; the difference is areal;" "the warmth was there, a deep diffuse warmth, and then it changed to a warmth centralized, punctiform and more like pain; the change was not sharp or sudden like a typical prick; it was more durative, like an increase in intensity."

B. "Burning warmth again! It differs from the actual burn in that it is brighter, more concentrated, smaller in area. It seems centered in an area of soft warmth, a little hard part that creeps in;" "except for an occasional dull pressure that comes in with the needle prick, the two experiences are qualitatively the same; the sting is less intensive than with full heat, *i. e.*, the burn is not so great."

All *O*s agree that the experience is a fusion, probably of warmth and prick, but it may be of sting (heat) and prick. At times it is a warm painful sting, at others a merely painful sting. In the latter case the warmth seems to become stingy, and this invariable stingy quality makes analysis difficult; it is not easy at times to distinguish it from warmth, and at others from pain. We are accustomed to regard sting as an intermediate quality which results from the simultaneous stimulation of warm and cold spots; but it is reasonably certain that no cold spots were stimulated in the present experiments. We found also that, with continuous stimulation of a warm spot by radiant heat, warmth changes to sting before burn appears; and in a few casual observations we were unable to distinguish this sting from that obtained by the simultaneous stimulation of a warm and a cold spot. The point would seem to be of considerable systematic importance, and we regret that we have been unable for lack of time to pursue it further.[4]

We are convinced, however, that warmth, particularly a warmth of good intensity, is qualitatively much more like sting than Cutolo has led us to suppose.[5] In our attempt to obtain an analysis of 'burn' we repeatedly asked our *O*s to compare the warmth and sting obtained by radiant heat. Some of the reports follow.

W. "Warmth, particularly at the higher intensities, is much like sting, and a high degree of warmth has the promise of sting. There are times when it is impossible to say when warmth changes to sting. The two qualities are, however, quite different, and the change from the one to the other is quick and smooth."

H. "As warmth got more intensive it became more alive and very weakly stingy. I could find no point where warmth changes to sting;" "there was a change from a rather soft, loose warmth to a more alive, penetrating, slightly stingy quality; I could not tell just where the change takes place; it is almost like a change in intensity;" "warmth itself seems to grow until it gets stingy."

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[4]We realize, in particular, that a radiant source, however carefully controlled, must be physiologically suspect. We hope to return to the problem with another technique.

[5]F. Cutolo, Jr., A Preliminary Study of the Psychology of Heat, this JOURNAL, xxix, 1918, 445ff. J. H. Alston, *op. cit.*, 312, gives some evidence for the view that heat is more like cold than warmth.

B. "At first an increase in warmth; then, as soon as warmth reaches a certain intensity, something else, a prickiness or sting, comes in which is more like pain than pressure; I should say that warmth is like pressure on one side and like sting on the other."

Our cases are too few to permit of a generalization; but we think it probable that further observation will place warmth in a pressure-pain continuum between pressure and sting (heat); no one who is familiar with warmth would hesitate to say that it is like pressure, and our *O*s unite in saying that, under our conditions, warmth is also like sting.

Conclusions. (1) The simultaneous stimulation of warm and pain spots may result in an experience which is variously called 'burning heat,' 'burn', and 'hotness'. Psychologically it seems to be a fusion of the prick quality of pain with either warmth or sting. It is best obtained when the warmth is focal and of good intensity, and the pain not too intensive. At similar intensities this fusion differs from 'actual burn' only in extensity.

(2) The continuous stimulation of a warm spot with radiant heat of constant intensity results in a series of qualities from warmth through sting and burn to pain.[6] The 'sting' obtained in this way is similar to (if not identical with) the quality obtained from the simultaneous stimulation of warm and cold spots, and it was found to have a qualitative likeness to warmth. There is some evidence, therefore, for the statement that warmth lies in a qualitative pressure-pain continuum.

[6]Warmth, sting (heat), and pain we take to be simple qualities, although we are not prepared to name the quality of the final pain.

BOOK REVIEW

Senescence: The Last Half of Life. By G. STANLEY HALL. New York, D. Appletion & Co., 1922, 518 pp.

Having recently retired from the presidency of Clark University, which in 1889 he founded with the financial assistance of Mr. Jonas G. Clark of Worcester, Mass., Dr. Hall decided to take stock of himself, to go over himself physically and mentally most carefully, and to ascertain what it really means to be old (since that is what he was, according to established chronological standards), and how closely it approaches the condition agreed upon for it by long-inherited public opinion. That there was a wide discrepancy between the two will be seen later. Moreover, having spent many years of his life as a genetic psychologist in the study of infancy and childhood, puberty and adolescence, and later of adulthood and sex maturity, he felt that completeness of programme required that he study the last two stages of human life, viz., senescence (from forty, or earlier for women) to the climacteric; and senectitude, which is post-climacteric, or old age proper. Accordingly, he visited half a dozen or more physicians and experts, only to find how little was their knowledge and how great their disagreement concerning this stage of life. The library yielded him some five hundred books and articles on the subject, and the present volume is the result of all these investigations, studies, and the reflections based upon a long life creatively active in many fields (in some of which he was the pioneer) and upon a memory most abundantly and systematically stored with facts gleaned from all the sciences cognate to his own and from history and literature. Thus, while most of the book is in the nature of a comprehensive compendium, it may be said to be almost throughout both original and autobiographical, because not only is the Hall-mark on all the materials that percolated through his mind, but they are transformed, enriched, and given a setting in a larger whole, of which the original authors probably knew next to nothing.

The opening sentence of the Foreword reads: "In this book I have tried to present the subjects of Old Age and Death from as many viewpoints as possible in order to show how the ignorant and the learned, the child, the adult, and the old, savage and civilized man, pagans and Christians, the ancient and the modern world, the representatives of various sciences, and different individuals have viewed these problems, letting each class, so far as I could, speak for itself."

The fundamental thesis of the book is that senescents have a most important function in the world, particularly in these troublous days; one which they have not themselves appreciated or measured up to, because they have not realized "what ripe and normal age really is, means, can, should, and now must do, if our race is ever to achieve its true goal." This function is to distil from the experience of the past the wisdom necessary for the wholesome life of the present, "to gather the fruitage of the past and to penetrate further into the future." And old age is perculiarly fitted to do this because "withdrawal from biological phyletic functions is often marked by an Indian summer of increased clarity and efficiency in intellectual work. Not only does individuation now have its innings but the distractions from passion, the lust for wealth and power, and in general the struggle for place and fame, have abated and in their stead comes normally, not only a philosophic calm but a desire to sum up and evaluate all of life's experiences." Furthermore, the old are disillusioned, they see through the shams and

vanities of life, hence are better guides in the realms of politics, civics, economics, social relationships, sex, marriages, and the family, as well as religion and philosophy. The world sorely needs the disillusionment, the perspective, "the aloofness, impartiality, and power of generalization that age can best supply.... These were the qualities that enabled the venerable Joffre to make his masterly two-week's retreat at the Marne. It was done against the will and wish of every one of his younger generals, who now admit he saved Paris and the war and that he was, in a sence, a true superman. The world never so needed the wisdom, which learning cannot give, that sees the vanity and shallowness of narrow partisanship and jingoism, of creeds that conceal more than they reveal, of social shams that often veil corruption, the insanity of the money hunt that monopolizes most of the energy of our civilization, and realizes that with all our vaunted progress man still remains essentially juvenile—much as he was before history began. .... What the world needs is a kind of higher criticism of life and all its institutions to show their latent beneath their patent value by true supermen who, like Zarathustra, are old, very old, with the sapience that long life alone can give. We need prophets with vision who can inspire and also castigate, to convict the world of sin, righteousness, and judgment. Thus there is a new dispensation at the door which graybeards alone can usher in. Otherwise humanity will remain splendid but incomplete. Heir of all the ages, man has not yet come into his full heritage. A traveller, he sets out for a far and supreme goal but is cut off before he attains or even discerns it. The best part of his history is yet unwritten because it is unmade."

"Perhaps in the large Aristotelian sense of the word politics is *par excellence* the work of and for old age.... From the patriarchs down the old have been the wisest shepherds of the people, and if young men have succeeded in diplomacy it is because they have been prodigies of precocity who have also devoted themselves to an intensive study of history, which is at best only a proxy for experience.... The old who are really so, who are not merely spent projectiles, relics, vestiges, or ruins that time has chanced to spare, do sometimes attain vision and even prophetic power, and their last real words to the world they are leaving are not like the insane babblings of the dying, which friends so often cherish, but are often the best and most worth heeding by their juniors of all their counsels. Some have told us that if the long awaited superman ever arrives, he will come by way of the prolongation of adolescence and others have said it would be by the fuller maturity of man in his prime. No doubt both these stages of life would be enriched and potentialized, but his first advent and his greatest improvement over man of today will be in the form of glorified old age. Nietzsche was right in making Zarathustra old and he himself was the overman whose message he brought to the world. He was intent on the future of man and not on his present, still less on his past."

Dr. Hall fully realizes that there is no virtue in old age as such. Indeed, there are many old who are anything but venerable, wise, or good. Such virtues as old age has are fully earned, not inherent. "Many of those who attain advanced years are battered, water-logged, leaky derelicts without cargo or crew, chart, rudder, sail, or engine, remaining afloat only because they have struck no fatal rocks or because the storms have not quite yet swamped them; or, to change the figure, because they have withered, not ripened on the tree.... A psychological senility that neither learns nor forgets is always a menace and a check instead of being, as true old age should be, a guide in emergencies. Thus we have not grown old aright and are paralyzed by a wisdom that is obsolete or barnacled by prejudice. How often it is said of reforms great and good that they are earnestly needed and entirely practical but must wait for their accomplishment until certain venerable but obstructive personages of a generation that is passing are out of the way, because they are prone to think the old good and the new bad, and that every change, therefore, must be for the worse. Thus many live too

long and undo the usefulness of their earlier years.... It is because there are so many such that the rôle assigned to the best of us is often so hard and so repugnant to our nature and to our needs.... The very little that is known of old age is so predominantly of its inferior specimens, its unfavorable traits and defects and limitations that some old have been prone to repudiate their years, while others are sorely tempted to accept a sham old age that is false to the best that is in us, instead of justifying and illustrating a better one."

"Ripe old age has been a slow, late, precarious, but precious acquisition of the race, perhaps not only its latest but also its highest product. Its modern representatives are pioneers and perhaps its task will prove to be largely didactic. It certainly should go along with the corresponding prolongation of youth and increasing docility in the rising generation if we are right in charging ourselves with the duty of building a new story to the structure of human life.... To repeat, our function is to finish a structure that still lacks an upper story and give it an outlook or conning tower from which man can see more clearly the far horizon and take his bearings now and then by the eternal stars."

The above quotations are from the eighth chapter, which is the concluding chapter dealing with old age. The next and last chapter deals with the Psychology of Death. The other chapters may be briefly summarized as follows: Chapter 1. *The Youth of Old Age.* At about 35 or 40 there comes a realization that the tide that 'drew us out the boundless deep' begins to 'turn again home'. This is the dangerous age. Both sexes realize that they face the bankruptcy of some of their youthful hopes, and certain temperaments make a desperate, now-or-never effort to realize their extravagant expectations and are thus led to excesses of many kinds; while others capitulate to fate, lose heart, and perhaps even lose the will-to-live. Quotes Osler, "the evil genius, the croaking Poe raven of this period," whose two fixed ideas were "the comparative uselessness of man above forty years of age and the uselessness of men above sixty years of age, and the incalculable benefit it would be in commercial, political, and in professional life if, as a matter of course, man stopped work at this age." Quotes E. G. Dexter, W. A. Newman Dorland and E. S. P. Haynes as disputing these conclusions, and adds: "If and so far as Osler is right, it is because man up to the present has been abnormally precocious, a trait that he inherited from his shorter-lived precursors and has not yet outgrown, as is the case with sexual precocity, which brings premature old age. Modern man was not meant to do his best work before forty, but is by nature, and is becoming more and more so, an afternoon and evening worker. The coming superman will begin, not end, his real activity with the advent of the fourth decade. Not only with many personal questions but with most of the harder and more complex problems that affect humanity we rarely come to anything like a masterly grip till the shadows begin to slant eastward, and for a season, which varies greatly with individuals, our powers increase as the shadows lengthen. Thus as the world grows intricate and the stage of apprenticeship necessarily lengthens it becomes increasingly necessary to conserve all those higher powers of man that culminate late and it is just these that our civilization, that brings such excessive strains to middle life, now so tends to dwarf, making old age too often *blasé* and *abgelebt*, like the middle age of those roués who in youth have lived too fast." Chapter 2. *History of Old Age.* Treats of the various ages attained by plants and animals, and the attitude toward (and treatment of) the aged among primitives, the ancient Hebrews, Greeks, Romans, the Middle Ages, and children. Everywhere, except among the Hebrews, the lot of the old was pathetic and even tragic, being ignored, neglected, abandoned, shorn of power and authority, put to death, and in times of famine eaten. The views of Cornaro, Bacon, Addison, Robert Burton and Jonathan Swift are quoted. Also Karl Pearson's theory of witchcraft as a revival of the ancient and widespread

matriarchate.... "In the eternal struggle of old people to maintain their power against the oncoming generations which would submerge or sweep them away, witchcraft on this view represents the very latest stage of a long and losing struggle of old women for place and influence who in the last resort did not scruple, handicapped though they were by ugliness, neglect, and contempt, to cling to the last remnants of their ancient prerogatives." Chapter 3. *Literature By and On the Aged.* Describes the various attitudes of literary men and women toward old age, followed by quotations from (or résumés of) the works of Margaret E. White, Harriet E. Paine, Amelia E. Barr, Mortimer Collins, Col. Nicholas Smith, Bryon C. Utecht, J. L. Smith, Sanford Bennett, G. E. D. Diamond, Cardinal Gibbons, John Burroughs, Rollo Ogden, James L. Ludlow, Brander Matthews, Ralph Waldo Emerson, Oliver Wendell Holmes, Senator G. F. Hoar, William Dean Howells, H. D. Sedgwick, Walt Mason, E. P. Powell, U. D. Wilson, D. G. Brinton, N. S. Shaler, Anthony Trollope, Stephen Paget, Richard LeGallienne, G. S. Street, C. W. Saleeby, Bernard Shaw. Chapter 4. *Statistics of Old Age and its Care.* Life is probably twice as long as it was three or four centuries ago, and is increasing more rapidly now than ever. The rate of progress is very variable in different countries, the maximum being in Prussia. Improvement is most in females and the rate of increase is accelerated perhaps four years a century on the whole, although during the last three quarters of the nineteenth century Irving Fisher thinks it has increased nine years. At least fourteen years could be added to human life by eliminating preventable diseases. Offsetting the increase in the length of life is the intensity of modern life and industry, which steadily reduces the age of maximal efficiency so that the handicap of years is felt earlier in life than formerly.... Not only is the average length of human life increasing as civilization advances, but so is the relative and absolute number of old people, and those who now attain 60, 70, 80 and above are on the average far more comfortable than ever before. However, conditions of life in the modern city, and especially since the Industrial Revolution, are far from being ideal for the old.... Nearly every civilized country makes some provision for its aged poor. There are old-age pensions, insurance, annuities, and various provisions made by private corporations, unions, fraternal orders, insurance companies, and philanthropic foundations. The U. S. government is the only one that has no retiring system or provision for old age for its employees, save for soldiers and judges of the Supreme Court.... In institutions for the old they suffer most from mass-treatment, for they are not a class but are hyperindividualized, and need most of all personal attention. There is the greatest diversity in food, regimen, and in most bodily and psychic needs.... All have their own tastes, aptitudes, habits, as well as mementos and keepsakes, which should always be respected, and every possible facility should be given not only for visits and correspondence but for current reading in order to maintain a larger surface of contact with the world without. The old thus constitute, in a sense, a privileged and even a new "leisure class." Chapter 5. *Medical Views and Treatment of Old Age.* There are no gerontologists as there are experts for women and children, and therefore each senescent must be his own physician. Body-keeping for the old is a very pressing and personal problem requiring much time and attention, and the methods that are successful differ so widely that the diet and regimen good for one might be dangerous, if not fatal, for another.... The view so commonly held, that heredity is the chief factor in longevity, is doubtless correct in general. But it is fatalistic and directly tends to lessen the confidence of hygienists and physicians in the efficacy of all their methods of prolonging life in the aged... The psychological effect of this dogma of the prepotence of heredity in determining the length of life is itself not only depressing but may readily become, as psychologists can best understand, a dangerous lethal agent with the old and cause those who have reached the span of years at which their forbears

died to succumb to their troubles with less resistance. Indeed it is one of the chief purposes of this volume to show that the old age problem is not merely economic, philanthropic, social, or even medical, but also, when all is said and done, perhaps chiefly psychological, and that the future welfare of the race depends upon the development of an old age due not chiefly to heredity but to better knowledge and control of the conditions of this state of life. Chapter 6. *The Contributions of Biology and Physiology.* Discusses succinctly the views on the cause of old age of Weismann, Elie Metchnikoff, C. S. Minot, Charles Manning Child, Jacques Loeb, Northrup, Carrel, Pearl, Pozzi, Claude Bernard, and the experiments of Brown-Séquard, Eugene Steinach, Serge Voronoff and their pupils on the rejuvenating effects of testicular fluids taken from young animals and injected into older ones; also of glandular transplantation and grafting; and concludes that "the only practical hope of easement from the hardships of senescence and for the postponement of death now tenable is that now arising faintly and tentatively that, some day, some mitigation of the terrors of old age and death may be found by glandular implantation or perhaps even by the injection of secretions of certain glands.... These studies are yet, however, in their infancy and it will be, at the best, a long time before we can know whether they are able to fulfill their promise to the human heart and to the will to live." Chapter 7. *Report on Questionnaire Returns.* Sent the following questionnaire to a few-score eminent and distinguished Americans. How and at what age did you first realize the approach of old age? To what do you ascribe your long life? How do you keep well, that is, what do you find especially good or bad in diet, regimen, interests, and personal hygiene generally? Are you troubled with regrets for things done or not done by or for you? What temptations do you feel, old or new? What duties do you feel that you still owe either to those about you or to the world? Is your interests in public, community, or in far future or past things, as compared with interest in persons and things right about you, greater or less than formerly? In what do you now take your greatest pleasure? Do you enjoy the society of children, of young people, adults, or those near your own age more or less than formerly? Would you live your life over again? Did you experience an "Indian summer" of revived energy before the winter of age began to set in? Do you rely more or less on doctors or find that you must study yourself and be your own doctor? Do you get more or less from the clergy and the church than formerly? Do you think or worry about dying or the hereafter more or less than formerly? Though respondents belonged practically to the same class, yet because of their age their individuation was so great as practically to preclude uniformity in their replies. As many respondents, almost so many different replies. Chapter 8. *Some Conclusions.* "To learn that we are really old is a long, complex and painful experience. Each decade the circle of the Great Fatigue narrows around us, restricting the intensity and the endurance of our activities....At sixty we realize that there is but one more threshold to cross before we find ourselves in the great hall of discard where most lay their burdens down and that what remains yet to do must be done quickly. Hence this is a decade peculiarly prone to overwork. We refuse to compromise with failing powers but drive ourselves all the more because we are on the home stretch. We anticipate leaving but must leave things right and feel we can rest up afterwards. So we are prone to overdraw our account of energy and brave the danger of collapse if our overdraft is not honored. Thus some cross the conventional dead-line of seventy in a state of exhaustion that nature can never entirely make good. Added to all this is the struggle, never so intense for men as in the sixties, to seem younger, to be and remain necessary, and perhaps to circumvent the looming possibilities of displacement by younger men. Thus it is that men often shorten their lives and, what is far more important, impair the quailty of their old age, so that we yet see and know

little of what it could, should, or would be if we could order life according to its true nature and intent. Only greater easement between fifty and seventy can bring right, healthful, vigorous senectitude, the services of which to the race constitute probably the greatest of our civilization today.... The seventieth is the saddest of all birthdays and if we 'linger superfluous on the stage', we feel that society regards us as, to some extent, a class apart; and so we instinctively make more effort to compensate our clumsiness by spryness and gently resist the kindly offices and tokens of respect to which the young incline or, perhaps more often, are taught to render the old.... It is not strange that one of our grievous dangers is patheticism. One who begins to suspect waning love on the part of those in his sphere may come to accept and even crave pity in its place.... There are fathers who, with no thought that they are selfish, monopolize the love and services of their daughters, and mothers who do the same of grown sons.... The old often feel a falsetto invalidism.... are particularly prone to develop peculiarities which tend to alienate those nearest to them, such as faults in table manners, neglect of toilet, soiled attire, objectionable noises in presence of others, pryingly overcurious, fault-finding, exacting, forgetful.... The old are subject to certain fluctuations, new in kind, degree, or both. Sleep is less regular.... appetites fluctuate and may readily become capricious.... There are alternations of moods.... greater dependence upon weather, climate, and seasons, winter being the hardest and spring the easiest.... even sex often does not decline and die without terminal oscillations in its course and in extreme cases apathy and aversion may alternate with abnormal erotic outbreaks dangerous alike to the health of the individual, to domestic happiness, and even to public morals.... There is mental starvation because the supplies of mental pabulum fail owing to the reduction of sensations and movements.... the days and years pass more slowly and fatigue sets in more quickly.... emotivity probably increases with years and most expressions of it, unless they become more sublimated, strongly tend to grow more crass and stormy. We were never more interested in things, persons, events, causes, in life itself. Slights rankle, neglect chills, attentions warm, affronts incense, and praise thrills us, and if we grow censorious, it is because our ideals of conduct and motive have become higher and purer and we are in a greater hurry to see them realized."

"Old age is called second childhood. This is all wrong for there is nothing rejuvenative about it. Childhood is the most active, healthful, buoyant, and intuitive stage of life; age, the least so.... The problem of personal hygiene looms up with new dimensions. In our prime we give little attention to health.... but now our credit at the bank of health begins to run low.... we must select the items of our dietary with discretion and self-restraint.... If current events impress and absorb us less, we knit up the past, present, and future into a higher unity." The remainder of this chapter, which is the best and most original in the book, was epitomized in the opening paragraphs of this review. Chapter 9. *The Psychology of Death.* "From infancy to old age the conceptions of death undergo characteristic changes in the individual not unlike those through which the race has passed.... Death is not only the king of terrors but to the genetic psychologist every fear is at bottom the fear of death, for all the scores of phobias that prey upon man are of things and of experiences that abate life.... The fear of death or of life-abatement for the individual is no whit less pervasive and dominant than are love and hunger, which are so often said to rule the world.... Man became man when he knew he must die, and to defer or escape death has been the basal motivation of all his culture.... Man may thus be redefined as the death-shunner. He does not and cannot begin to realize how much he fears death and dreads it now and always has .... The most essential claim of Christianity is to have obviated through its doctrines of the other world, resurrection, and personal immortality,

the fear of death and made the king of terrors into a good friend, if not into a boon companion.... but this belief persists only as a dead article of faith which men no longer live by. It is a desiccated herbarium specimen and not a living plant...."

The psychical factors that have overdetermined the hope-wish of personal immortality are as follows:

(1) The desire to be remembered and esteemed by survivors.

(2) The desire to do things that will affect those who survive or will perpetuate our will and works to those who know little or nothing of us or of our name. "Jubal's fame and art filled all the sky, while Jubal lonely laid him down to die," supremely happy in the thought that he had done the race a great service.

(3) The third killer of the death-fear is children and posterity.

(4) The need of another world and life to compensate for the wrongs and imperfections of this one. However, our actual *modus vivendi* is as if another life did not exist and death were the end. No priestcraft can longer make men content with misery here in the hope of compensation hereafter. All make the most and best of this life as if it were all they were sure of, and the motto of most believers is, "One life at a time and this one now."

(5) Another killer of the death-fear is the discovery of physics and chemistry that "death is not only non-existent but inconceivable.... Matter is not only not dead but more intensely active than mind. Transformations take place, but not a single ion dies or is lost."

(6) Mysticism, pantheism, and noetic theories of immortality teach the same thing concerning the individual soul. It does not die but becomes absorbed into the All-Soul.

(7) Philosophy too, from Plato on, has advanced many arguments to prove the immortality of the soul.

"But the fear of death and the forms of mitigating this fear are chiefly because man still dies young. If we had experienced and explored senescence fully we should find that the lust of life is supplanted later by an equally strong counter will to die. We should have no immortality mania for we should be satisfied with life here without demanding a sequel to it. Our present dreams of all forms of post-mortem existence would become a nightmare. True macrobiotism means not only more years but completeness of experience, absence of repression and limitation. Had we lived out the whole of our lives and drained all the draughts of bitter and sweet that nature has ever breathed for us, we should feel sated.

"The fact is, man is now cut off in his prime with many of his possibilities unrealized. Hence he is a pathetic creature doomed to a kind of Herodian slaughter and because he has dimly felt this he has always cried out to the gods and to nature to have mercy. He has imagined answers to the heartrending appeals he shouted into the void; if man dies shall he live again? and on the warrant of fancied answers has supplemented this by another life, which, when psycho-analyzed in all its processess, means only that he has a sense that the human race is unfinished and that the best is yet to come. And so it is. Man's future on this earth is the real, only, and gloriously sufficient fulfillment of this hope. It will be found only in the prolonged and enriched life of posterity here. The man of virtue will realize all desires and live himself completely out so that nothing essentially human will be foreign to his own personal experience.

"Thus the wish for and belief in immorality is at bottom the very best of all possible augurs and pledges that man as he exists today is only the beginning of what he is to be and do. He is only the pigmoid or embryo of his true and fully entelechized self. Thus when he is completed and has finished all that is now only begun in him, heavens, hells, gods, and discarnate ghosts will all fade like dream fabrics or shadows before the rising sun. All doctrines of another life are thus but symbols and tropes in mythic form of the true superman as he will be when he arrives. The great hope so many

have lived and died by will be fulfilled, every jot and tittle of it, not in our own lives but in the perfect man whose heralds we really are without knowing it. Deathbed visions will come true more gloriously than the dying thought. They hunger for more life but the perfect man will die of satiety passing over into aversion and the story will be completed not in a later number but in this."

Of course the long and painstaking studies that produced the two-volumed *Adolescence, Educational Problems*, and *Jesus, the Christ* made possible the production of this last volume in considerably less time and with less labor than were expended upon them; and while the specific gravity, so to speak, of the *Senescence* is not as high as that of its predecessors, nor probably was intended to be, there is no mistaking its authorship from first page to last. Though struck off quickly as a minor work, it shows throughout the master's touch, and it can be safely said that there is in no language a work on the subject so comprehensive and discriminating as this one. Moreover, if the function and service of the supersenescents of the future will be to synthetize, "to draw from accumulated experience and knowledge the ultimate and especially the moral lessons of life—in a word, to sum up in a broader view the net results of all we have learned of the *Comédie humaine*", then has this macrobiotic author proved himself a splendid forerunner of the species he has so ably analyzed and described. In the language of the street, *Senescence* is a big book by a Grand Old Man.

JOSIAH MORSE

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NOTES

An Emendation

I have always been puzzled by the sentence which, in the first edition of the *Physiologische Psychologie*, leads over from the discussion of Consciousness to that of Attention. The first clause is awkwardly worded; the explanation, so far as I can see, does not explain; and the psychology is assuredly not that of the Wundt we know. One may say, of course, that Wundt did not always write impeccable German; that what is a non-sequitur to us may to him, with his associations thick upon him, have been a good argument; and that in 1874 he had not settled down to any very stable psychology. One may say, also, that the whole matter has today only an antiquarian interest. I have said all these things to myself, and have remained unsatisfied. Now, at last, I think I have the key to the passage: I believe that a slightly different sentence stood in Wundt's manuscript, and that the sentence as we read it embodies a correction made while he was going over his paged proofs.

The printed sentence is as follows. "Indem das Bewusstsein in der Synthese der Empfindungen und in der Association der Vorstellungen sich selbst als ein thätiges erfasst, entsteht jene Aeusserung desselben, welche wir *Aufmerksamkeit* nennen." "In the synthesis of sensations and in the association of ideas consciousness apprehends itself as active: hence (*i.e.*, by way of this apprehension of its own activity) arises that manifestation of consciousness which we call *attention*." I believe that what Wundt originally wrote is as follows. "Indem das Bewusstsein in der Synthese der Empfindungen und in der Association der Vorstellungen ein thätiges Erfassen aufweist, entsteht jene Aeusserung desselben, welche wir *Aufmerksamkeit* nennen." I cannot, naturally, be sure of the verb; but my hypothesis requires a short verb meaning 'shows,' 'exhibits,' and *aufweist* does very well. "In the synthesis of sensations and in the association of ideas consciousness displays an active apprehension: hence (*i.e.*, by way of this activity of apprehension) arises that manifestation of consciousness which we call *attention*." This I take to be good German, sound argument, and true Wundtian doctrine. I believe that Wundt changed it, on the printed page, to the sentence that appears in the book. Why should he have made the change?

One must remember the circumstances: that Wundt had been working under pressure of time, that he was very tired, that the whole theory of apperception—at any rate, so far as this book is concerned—was an afterthought. I think, then, that Wundt, when he read his sentence in proof, had an uneasy feeling that it was not adequate to his intended transition. He was introducing a second systematic thread into his psychology; sensory integration was henceforward to be paired with attention. He had just been saying that consciousness is, essentially, synthesis; now he was to break away from synthesis. But was the original sentence sufficient? Might not an unfriendly critic find fault with its logic? "Indem das Bewusstsein in der Synthese der Empfindungen und in der Association der Vorstellungen ein thätiges Erfassen aufweist,"—might not the unfriendly critic finish it by writing: "entstehen diejenigen Aeusserungen desselben, die wir Sinneswahrnehmung und Associationsgesetz nennen,"—something of that sort? But then one would be back again in synthesis, and the step to attention would still remain to be taken.

The imaginary critic would, no doubt, have been wrong,—wrong, and either careless or obtuse. Wundt, however, was very tired, as I have said,

and was also exceedingly anxious to make his point. The conscious activity which underlies attention must be thrown into sharp contrast with the conscious act of synthesis; the introductory clause must differentiate as well as connect. And so I think that Wundt, whose logical acumen had been dulled by the same fatigue that made him oversensitive, sought at the last moment to clinch his argument by raising the activity of attention to a higher level of consciousness. If synthesis occurred on one plane, and apperception on another, there could be no doubt about the twofold basis of his psychology; and consciousness itself, as somehow active on both planes, would keep the psychology unitary. A muddle, —but, under the circumstances, a very natural muddle!

That is my hypothesis: and, if the reader is interested to prove it, I ask him not to do as I have done in this Note, not to confine himself to the structure and logic of the single sentence on p. 717, but rather to consider the whole make-up of Wundt's ch. xviii. If he is still not persuaded, I ask him to turn to the corresponding passage of the second edition.

E. B. T.

Serial Exposition of Wall-Charts

It is often convenient to be able to show a set of charts at the same place on the lecture-room wall,—whether because space is limited, or because all charts must be seen from the same angle, or because the charts in question form a series to be shown in a fixed order. The following device may be worth recording.

The foundation of the whole arrangement is a heavy wooden frame, like a picture frame; it may be of any convenient dimensions. This frame may carry the chart to be exposed last in order. The separate charts of the series are of heavy paper, to the top corners of which are attached rings of thin wire, extending approximately their full diameter (say, 2 cm.) above the edge of the paper. If these charts are of the same width, it is obvious that two supports will hold them all.

The distinctive feature of our arrangement is the substitution of screws rotated by pulleys for the ordinary hooks. The screws pass through the upper strip (2.5 cm. thick) of the wooden frame. Their forward ends (points) are blunt; their backward ends (heads) are fastened to pulleys, which are set flush with the back of the strip. If a spindle is turned down on the head of each screw, the shoulder of the spindle prevents the screw from passing out of the strip at the back, and the pulley at the back prevents the screw from coming forward. The screws are cylindrical, like machine screws; the grooves, however, are relatively deep and broad (about 3 by 3 mm.) and the interspaces are broad and flat (same dimensions). These dimensions are important, because the wire rings must ride smoothly in the grooves, without jumping out, and must be separated along the screw by sufficient space to prevent their interlocking.

On the back of the right-hand side-strip of the frame, low down so as to come within reach of the lecturer, are two pulleys fastened one above the other. A belt passes over the upper pulley to the screw-pulley at the top-right of the frame, and another belt passes over the lower pulley to the screw-pulley at the top-left.

The charts are hung on the frame in the reverse order of that in which they are to be shown, every ring riding in its own groove on the upper side of the screw. To remove a chart, the lecturer pulls the lower belt, rotating the left-hand screw one full turn. Every left-hand ring is carried forward in the groove, until the front ring drops down over the end of the screw. The lecturer catches the dropping chart, and pulls the upper belt, thus releasing the right-hand ring. After a few practice-trials, the charts come away smoothly and noiselessly.

For sudden or total exposures an ordinary roller-shade may be added. The fixtures are set in the front of the top strip of the frame, outside the screws, and the shade is let up and pulled down by a cord in the regular way. After a chart has been shown, the shade is drawn down, and the chart released from its supports under cover of the shade; then the shade is shot up for the next exposure.—

We have found that a light flat spring, resting along the upper surface of the screw, serves to hold the rings securely in place when they are set in the grooves. We have not found it possible to use a single pulley-belt; the slipping of the belt always, in our experience, allows one screw to rotate further than the other; with the result that the discharge of a ring over the end of one screw may happen twice before the other has discharged its first ring. We have tried to simplify the whole arrangement by using one central ring and one central screw; but, with charts of the size that we require, the twofold support is more satisfactory.

H. G. Bishop

Cornell University

The Titchener Commemorative Volume

A limited number of copies of the Titchener Commemorative Volume are left in stock. Since the sales to date have more than paid the costs of the edition, the Committee in charge of publication have decided to offer these remaining copies to psychologists at the reduced price of two dollars, postpaid. The proceeds of their sale, together with the balance already in hand, will be funded, and the interest will presently be used to establish a prize for meritorious work in experimental psychology.

The volume, which consists of 337 pages of the style and size of the pages of The American Journal of Psychology, contains eighteen studies in various departments of psychology, dedicated to Professor Titchener by colleagues and former students on his completion of twenty-five years of service to Cornell University.

Orders may be sent to D. R. Knight, Morrill Hall, Cornell University, Ithaca, N. Y.

M. F. Washburn
W. B. Pillsbury
K. M. Dallenbach

A New Psychological Periodical

We have received the first three parts of a new psychological magazine, *Psychologische Forschung: Zeitschrift für Psychologie und ihre Grenzwissenschaften*, edited by K. Koffka (Giessen), W. Köhler (Berlin), M. Wertheimer (Berlin), K. Goldstein (Frankfurt a. M.) and H. Gruhle (Heidelberg). The contents are as follows.

Bd. i., Heft 1, 2. 1921. W. Köhler. Zur Psychologie des Schimpansen. M. Wertheimer. Untersuchungen zur Lehre von der Gestalt. i. Prinzipielle Bemerkungen. D. Westermann. Tod und Leben bei den Kpelle in Liberia. K. Koffka. Beiträge zur Psychologie der Gestalt. v. Untersuchungen über Bewegungs- und Verschmelzungsphänomene, von P.C ermak und K. Koffka. E. M. von Hornbostel. Ueber optische Inversion. W. Fuchs. Eine Pseudofovea bei Hemianopikern. Referate: E. Rubin, Synsoplevede Figurer; Studier i psykologisk Analyse.

Bd. i., Heft 3, 4. 1922. K. Lewin. Das Problem der Willensmessung und das Grundgesetz der Assoziation, i. A. Pick. Störung der Orientierung am eigenen Körper: Beitrag zur Lehre vom Bewusstsein des eigenen Körpers. G. Marzynski. Sehgrösse und Gesichtsfeld. K. Koffka. Beiträge zur Psychologie der Gestalt. vi. Ueber die Veränderung von Vorstellungen (Gedächtnis und Gestalt), von F. Wulf. J. Borak. Ueber die Empfindlichkeit für Gewichtsunterschiede bei abneh-

mender Reizstärke. W. KÖHLER. Ueber eine neue Methode zur psychologischen Untersuchung von Menschenaffen. Referate: A. Argelander, Beiträge zur Psychologie der Uebung, i. Uebungsfähigkeit und Anfangsleistung.

Bd. ii., Heft. 1, 2. 1922. E. LAU. Versuche über das stereoskopische Sehen. K. KOFFA. Beiträge zur Psychologie der Gestalt. vii. Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten, von E. Lindemann. T. W. DANZEL. Die psychologische Bedeutung magischer Bräuche. K. LEWIN. Das Problem der Willensmessung und das Grundgesetz der Assoziation, ii. E. M. VON HORNBOSTEL. Bemerkungen zu einer "Grundfrage der Akustik und Tonpsychologie." Kleine Mitteilungen: Aus dem psychologischen Institut der Universität Giessen; Aus dem psychologischen Seminar der Universität Kiel. Referate: E. Kretschmer, Körperbau und Charakter; J. L. Entres, Zur Klinik und Vererbung der Huntingtonschen Chorea.

ERRATUM

In the article by H. E. Starr, "The Hydrogen Ion Concentration of the Mixed Saliva considered as an Index of Fatigue and of Emotional Excitation, and applied to a Study of the Metabolic Etiology of Stammering," published in the preceding number of the JOURNAL, the words *anion* and *cation* have by a printer's error been interchanged in two places: (1) last line of p. 397 and first of p. 398, and (2) lines 15, 16 of p. 398. We ask our readers to make the necessary correction.

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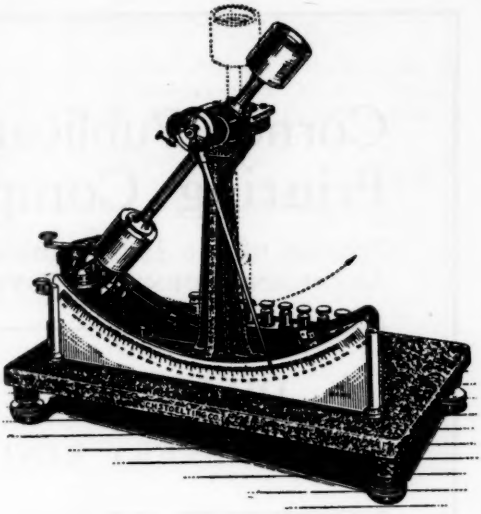

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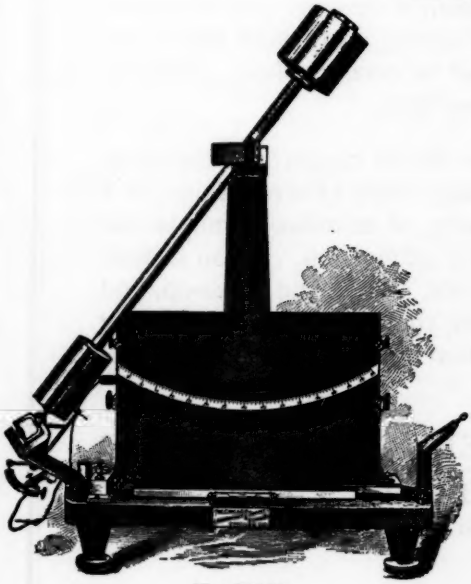

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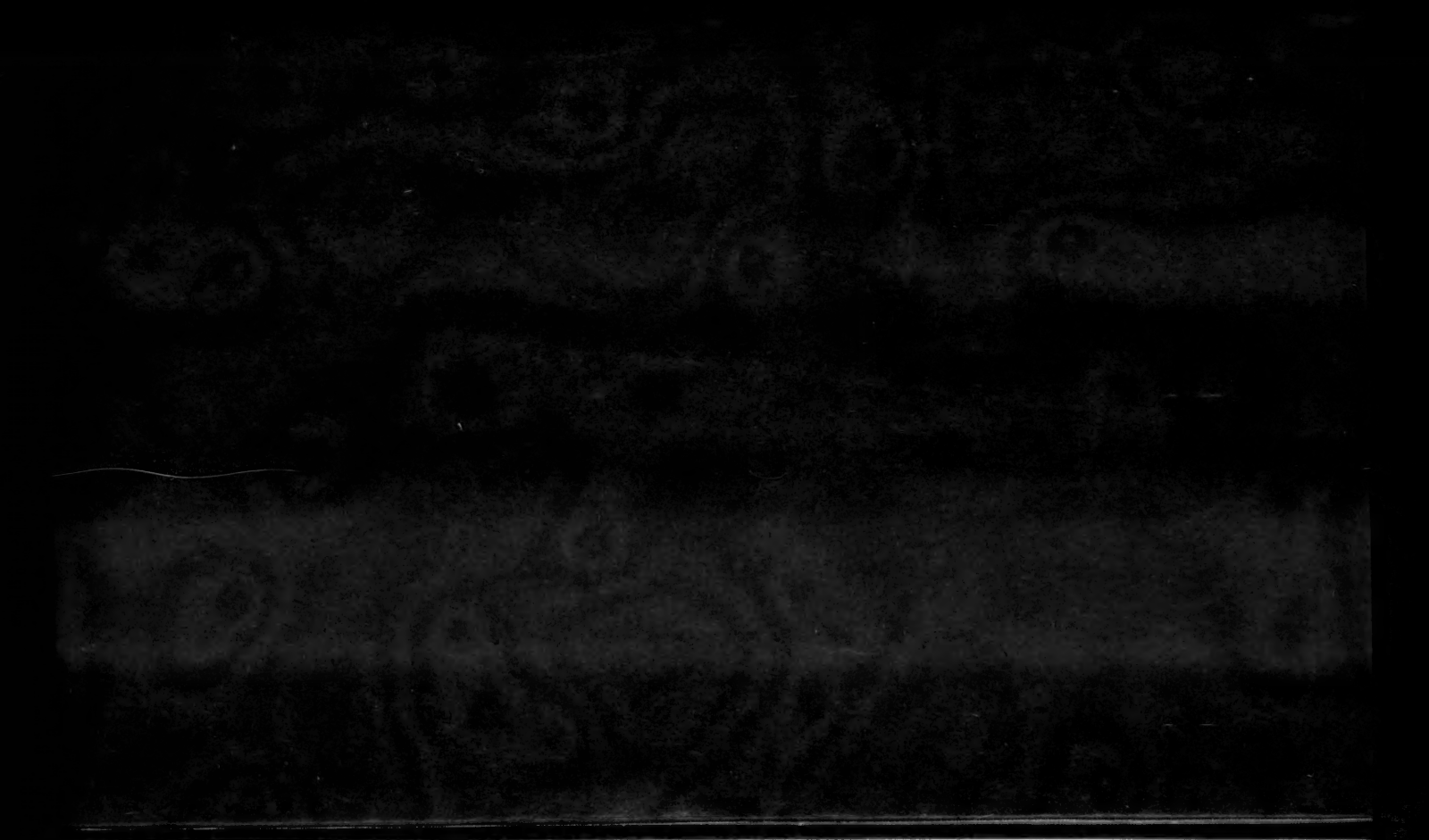
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